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THE
ANALYSIS,
TECHNICAL VALUATION, PURIFICATION,
AND
USE OF COAL GAS.

BY

THE REV. W. R. BOWDITCH, M.A., F.C.S.,

INCUMBENT OF ST. ANDREW'S, WAKEFIELD.

With Numerous Illustrations.

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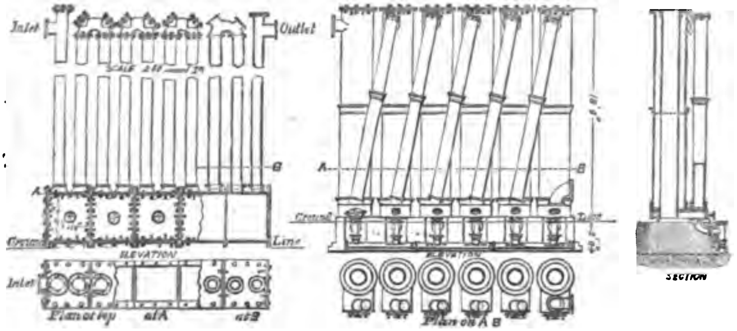
THE
ANALYSIS AND USE OF COAL GAS.

CHAPTER I.

CONDENSATION OF GAS.

When coal is put into retorts for distillation it contains water, and a considerable quantity of additional water is *formed* during distillation. This water takes up ammonia, sulphuretted hydrogen, carbonic acid, cyanogen, &c., and also a part of the volatile light-giving hydrocarbons to which coal gas owes much of its luminosity.

During the distillation a quantity of impure hydrocarbon oils are formed, which are well known under the name of gas tar. Both this tar and the ammoniacal liquor must be separated from gas when it leaves the hydraulic main, or the whole of the purifying and storing apparatus would become filled with them. They are removed by a process of cooling the gases and vapours, commonly known by the name of "condensation." The apparatus employed for this purpose is called a "condenser," and the forms in most general use are shown in the annexed figures.



The condenser is essentially a quantity of iron pipes, which are in communication at the bottom with an iron chest-like receiver, into which the tar and ammoniacal liquor fall, and from which they run into subterranean stores, like large wells. The condenser acts by transmitting heat from the gas and vapours within to the external air or water in contact with its outer surface, and by this the gases and vapours are cooled so as to separate the one from the other. The vapours become fluids, and fall into the wells; and the gases pass on to be purified, stored, and used. About ten feet of condenser-surface for each thousand feet of gas made per day is allowed by the best engineers, but very wide departures from this proportion are familiar to all who are acquainted with gasworks as they exist in fact.

Hitherto *principles of science* seem to have had but little connexion with condensation; but if the gas were reduced to the "proper temperature" before it passed on to the scrubbers or purifiers, the true object of purification has generally been considered as accomplished. An engineer has thus expressed the general aim and opinion: "Still further to separate all condensable vapours before allowing the gas to pass to the purifiers, a set of condensers, or coolers, is provided, through which the gas is made to circulate until it is reduced to a temperature bearing some approximation to the surrounding atmosphere." There could be no more mistaken practice than the one commonly adopted, for it is in the power of most engineers to make a considerable difference in the illuminating power of all the gas they manufacture, by the adoption of a better mode of condensation. The increased illuminating power obtained by a superior method of condensation is obtained almost without cost for material, whereas, when gas is improved by the use of cannel coal, the cost for material is considerable. If a quantity of the lighter hydrocarbons are withdrawn from tar and sold with gas, the tar will probably become of less value, and the difference between its present value and its value in the future (if reduced) might be called the cost of the material for producing the extra light; but, even so, the extra light would be produced far more cheaply than by other means at the command of a gas manufacturer.

So few people are conversant with the principles involved in this question, that I propose to consider some of its details, in order to make the matter clearer. The proper object of condensation is the removal from gas of substances produced in the distillation, which, for some reason, are not useful for the purpose of illumination, and the retention in the gas of all the substances which are useful as illuminants and can be distributed with the gas. Water and most of the compounds which the water holds in solution are useless, and cannot be distributed with the gas. These, therefore, should be removed. The heaviest hydrocarbons (tar) are useful as illuminants, but cannot be distributed with the gas, and therefore they should be removed. But the gases and the lighter hydrocarbon vapours are useful, and can be distributed; and therefore no more of these should be removed than is absolutely necessary to effect a proper removal of the compounds which must be removed, because they are useless as light-givers, or cannot be distributed. Of all substances, the most useful as light-givers, when burnt with gas, are the vapours of hydrocarbons, and therefore every pains should be taken to keep in the gas the largest possible quantity of these vapours. All the light hydrocarbons, known as "coal naphtha," have been obtained by the distillation of gas tar, and consequently have been separated from gas by the process of condensation now used in our gasworks. If gas be passed over this naphtha, it gradually disappears, and the gas which passes over the naphtha acquires considerably greater illuminating power than it possessed before its passage over the naphtha. This simple experiment shows, first, the illuminating value of the naphtha which was removed from the gas during the manufacturing process, and, secondly, the power of the gas to take up and transport so much of the naphtha as to acquire considerably greater illuminating value, when the naphtha is presented to it alone, without the presence of any other substance which acts antagonistically to the gas. Some published experiments made in the City Laboratory show that four grains of naphtha added to a cubic foot of common London gas increase its illuminating power 25 per cent., and 10·77 grains added to a cubic foot of the same gas increase its illuminating power 77 per cent., so that a gas manufacturer's aim should be to keep in his gas the largest portion of this vapour which his coal will furnish, and he can distribute. I never yet saw common gas which would not take up, and carry during its distribution, four or more grains per foot of suitable hydrocarbon vapour. At present most manufacturers of gas take the best means with which we are acquainted for removing hydrocarbon vapours from gas. They cool their gas and hydrocarbon vapours down to 60° or 70° F. (and in some cases lower), as quickly as they can, and the form of condenser which will do this most quickly and certainly is the one generally preferred. No suspicion seems to cross their minds that they are enabling the tar (*i.e.* the heavier oils) to dissolve out from the gas, and carry down with it the vapours of light hydrocarbons which they ought to retain in their gas with the utmost sedulity. To understand this it is necessary to understand something of the nature and properties of the complex substance called gas tar. The following Table does not profess to be complete, but it will suffice to give as much information as is necessary to enable an attentive person to understand the subject of "condensation," though he may have no chemical knowledge. It is arranged according to the boiling-points of the substances contained in it.

This Table includes all the heavy hydrocarbons which form tar, as well as the lighter and more volatile ones, such as benzole, and it may be objected by some persons that heavy oils cannot be carried by gas. This is a mistake. I have obtained from the gas supplied to my house a quantity of oil so heavy, that is, of such high boiling-point, that a Florence flask in which it was distilled, softened and partially lost its shape before the oil could be driven over. These heavy oils were obtained by passing the gas over carefully fractionated

pure light coal oils. The gas deposited a portion of its heavy oil, and took up the light instead. I have likewise fractionated oils obtained from the syphon wells of street mains, and have found them to contain the whole series of hydrocarbons, up to the very heavy ones I have mentioned above. I have likewise passed gas at common atmospheric temperatures over hydrocarbon oils boiling from 220° F. to 280° F., and the *whole* was taken up; so that, in any inquiry as to the power of gas to take up and carry the hydrocarbons which form tar, we must not confine our view merely to benzole, and other hydrocarbons of still lower boiling-point, but must extend it at least to those which boil as high as 400° F.

Mere inspection of this Table will show the most unskilled person that benzole will boil and pass away as vapour at 36° F. below the boiling-point of water, whereas another neutral fluid hydrocarbon, caproyle, requires to be heated to more than 183° F. above the boiling-point of water before it will boil and pass away in vapour like the benzole. It seems obvious then that, if we wish to keep the vapour of benzole in gas, and to separate the caproyle from it, we ought to keep all the substances produced in distillation at a temperature slightly above 176° F. and below 395° F. It seems as obvious that if the whole products of the distillation are cooled down quickly to 60° F. or 70° F., that all, or nearly all, the substances condensable at that temperature will be condensed and removed from the gas as fluids or solids, according to their habitudes. Mere inspection of the Table would lead to this; but it would not lead unskilled persons further. Those acquainted with the subject are aware that it is capable of leading much further, and to very useful conclusions. It is

N	Specific gravity.	Specific gravity of vapour.
Hexelene	. . .	2·911
Heptylen	·714	3·397
Benzole ..	·882	2·698
Butyle ..	·705	4·070
Toluole ..	·872	3·182
	·955	
Cumole ..	·866	3·666
Amyle ..	·774	4·992
	·96	
Cymole ..	·853	4·151
	·921	
	1·028	
	1·065	
Caproyle	·756	5·897
Naphthal	1·048	4·528
	1·081	4·519
	1·072	5·14
Paranaph	. . .	6·78
Metanaph		
Chrysene		
Pyrene		
Paraffin		

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* Schorl *al of the Chemical Society*, 2nd series, vol. iv., p. 356.

[*To face page* 6.

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* Schorl al of the *Chemical Society*, 2nd series, vol. iv., p. 356.

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known that mere distillation is incapable of separating the various classes of hydrocarbons from each other; that those of low boiling-points carry over with them portions of others of high boiling-points, while the latter retain some of the former, although they have been heated considerably above the boiling-points of the lighter compounds. So much is this the case, that I never yet examined a sample of commercial benzole without obtaining naphthalin from it, though the latter boils at 410° F., and the former at 176° F.; and I have no recollection of meeting with a sample free from some portions of the less volatile fluids, caproyle, cymole, &c. These substances dissolve in, or unite chemically with, each other (I think there is strong evidence that the latter is the fact), and we must resort to a carefully conducted artificial process to separate them. Mr. Warren's condenser will explain how this is best accomplished. That gentleman uses an ordinary condensing coil of pipe, but instead of keeping it cool he keeps it hot, and instead of sending the vapours into the top, and allowing the condensed fluids to descend, he sends them into the bottom of the coil and compels them to ascend. The coil is heated to the boiling-point of the volatile substance which he wishes to separate, and the mixed vapour from the retort is sent into the lower part of the coil. In its passage upwards the *vapour* is distilled, the more volatile compound passes on to be condensed into fluid in the ordinary manner, and the less volatile ones condense in the heated pipe and fall back into the retort. This beautiful arrangement points out what ought to be done in condensing gas. The hydraulic main ought to be kept at a proper temperature, and to be prolonged sufficiently to allow the less volatile bodies to condense and pass away to the tar well, and the more volatile to go forward with the gas; or, what is much better, a special apparatus, kept at a regulated temperature, should be interposed between the hydraulic main (in this case kept hot, of course) and the purifying apparatus, in which the gas and lighter hydrocarbons might be separated from the heavier bodies, and so a larger proportion of hydrocarbon vapour might be distributed with the gas; in other words, the gas might be made more illuminating. If thus separated, there would be no difficulty in distributing all the light hydrocarbon vapour which the tar would give up, as may be proved by the well-known process of pouring naphtha into the mains, which the gas picks up on its passage and carries to the consumers. Gas, at common temperatures, can take up and carry a considerable quantity of the vapour of the lighter hydrocarbons, *if they be brought into contact with it after having been separated from the heavy ones;* but the power of the heavy to carry down the light is greater than that of gas to carry them on, and hence all fall together in the present system of condensation, to the great detriment of the gas. The loss of light which gas suffers upon being passed through cold heavy oil, and the inability of the same oil when heated to act upon it, will illustrate this. I have passed gas through heated heavy coal oil boiling about 400° F., and have found its illuminating power identical with its illuminating power before it was passed through the oils. It gave up nothing to them, and it took up nothing from them. I have also passed gas through the same kind of oil at ordinary temperatures, and have had its illuminating power reduced to less than 1 candle for a consumption of 5 ft. an hour, whereas before passing the gas through the oil it had an illuminating power of

13 candles. The cold oil had dissolved out of the gas nearly all its illuminating matter. In the present method of condensation this is imitated in a great measure. All the oils, the water, and the gas are cooled together, and thus an opportunity is afforded to the heavy oils to dissolve out the light ones from the gas, whereas the heavy oils should have been condensed, first, at such a temperature that they could not damage the gas, and then the gas and the lighter vapours should have been cooled to the temperature proper for purification.

This method of condensation would probably tend to lessen the deposition of naphthalin in gas mains, which now causes so much trouble. It is well known that naphtha poured into mains obstructed by naphthalin will dissolve it, and that deposition cannot occur at ordinary temperatures in the presence of a considerable quantity of the vapour of the lighter hydrocarbons. It seems probable, therefore, that the presence in gas of as much hydrocarbon vapour as the suggested process would keep in it would enable it to uphold its naphthalin in a state of vapour by diminishing the tendency of the naphthalin to crystallise. If it will accomplish this, it will be a great boon to those engineers who use Newcastle coal and very high heats. I should expect it to contribute to this end in another way. The heavier compounds which form tar have a strong affinity for naphthalin at high temperatures, and as they condense are apt to carry down the naphthalin with them. I should expect, therefore, that by the fractional condensation which I have suggested so much naphthalin would be removed from gas by the heavier compounds which would separate in the "hot condenser," that the vapours of the lighter oils would be able to uphold

pure light coal oils. The gas deposited a portion of its heavy oil, and took up the light instead. I have likewise fractionated oils obtained from the syphon wells of street mains, and have found them to contain the whole series of hydrocarbons, up to the very heavy ones I have mentioned above. I have likewise passed gas at common atmospheric temperatures over hydrocarbon oils boiling from 220° F. to 280° F., and the *whole* was taken up; so that, in any inquiry as to the power of gas to take up and carry the hydrocarbons which form tar, we must not confine our view merely to benzole, and other hydrocarbons of still lower boiling-point, but must extend it at least to those which boil as high as 400° F.

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Cymole .	·853	4·151
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Paraffin		

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it is stated, had been an assistant to Winsor, was very near introducing an excellent system of purification. When his gas was produced, he tried to purify it by passing it *over* lime; but whether the lime was slaked or not I am not sure. Unslaked lime would be nearly useless; but even if he were so unchemical as to use his lime unslaked, the water introduced with the gas would slake it partially, so as to render it useful in a measure. The mistake he made was in passing the gas over and not *through* the lime; for although when passed over it the particles of gas which happened to come in contact with the lime would be purified, yet the major part would pass away from the purifying vessel without coming into contact with the lime. This rendered the process practically useless for removing the sulphuretted hydrogen. In addition to lime, he used the fixed alkalis, alkaline earths, and also the oxides of iron, manganese, zinc, copper, lead, &c. If he had passed the gas *through* the purifying material, he would have done at that early period of the history of gas-lighting nearly all which was done up to 1860.

In 1808, Winsor made a great improvement, for he passed the foul gas through a vessel which was perforated with small holes, and filled with lime-water or cream of lime, which he said withdrew the sulphur and other offensive substances. He also proposed to put lime into his retort, but he made the mistake of placing it at the bottom instead of at the top of his charge, a mistake which it seems difficult at this day to account for. There is no question, however, that his experiments brought the impurities in the gas better into contact with the milk of lime than they had been brought before, and consequently what he did was a great step in the history of purification. In the year 1808, Henry and Clegg put up still better purifying apparatus for using milk of lime; and Taylor patented his mode of purifying gas made from bones and animal matter (I cannot see why he should not also have included that made from coal) from sulphuretted hydrogen by lime and water, and then from ammonia by dilute sulphuric or hydrochloric acid.

In 1815, Clegg's practical skill improved the purification greatly. He introduced apparatus to stir up the milk of lime, instead of allowing the lime to settle at the bottom of the vessel as it had done up to that time. By stirring up the lime the gas was brought more effectually into contact with it, and consequently was better purified than it had been before. In fact, he really effected a very complete purification of gas from sulphuretted hydrogen and carbonic acid; and if it had not been for one serious drawback, the purification by wet lime, well agitated, would have remained until this day the best process for removing the particular compounds from gas which it is adapted to remove. This drawback was the large amount of refuse left by the process, and the pressure it threw upon the retorts. The latter could now be overcome by using an exhauster; but the former was invincible, and sufficed to cause rejection of the process. The refuse was called "Blue Billy" by the workmen, and Blue Billy can hardly be mentioned in the presence of old gas engineers without awaking recollections of troubles and prosecutions which they have long since consigned to forgetfulness.

We have lately heard much about ammonia as an all-powerful substance for removing sulphur compounds from gas, and more than one person has shown a disposition to claim its use as an original discovery. It will surprise many to be told that so long ago as 1817 a person named Wilson actually suggested and patented this very substance as a purifying agent. He tells us that he set free ammoniacal gas from gas liquor by means of lime, and mixed the ammoniacal gas so freed with coal gas between the retorts and the gasholder, and then caused the mixed gases to pass through a perforated plate placed under the surface of water; that is, he made a solution of ammonia, and washed the gas therein, to remove sulphuretted hydrogen and carbonic acid, together with cyanogen and certain other compounds, of which, I dare say, he knew nothing, but which would be removed notwithstanding.

A great practical advance in purification was made in this year by Phillips, of Exeter, who introduced what is commonly known by the name of "dry lime purification," but for which improvement the use of gas must have been greatly restricted. We are indebted to this improvement for the non-production of that offensive substance called "Blue Billy." Phillips freed gasworks from this nuisance by lessening the amount of water employed with lime when it was used as cream in wet-lime purifiers. He slaked the lime with water, and afterwards added to it just so much more water as made it moist enough to form a ball in the hand upon gentle pressure, and in this state it was used upon sieves or trays, and the gas passed through it. He thus describes his invention: "I take any quantity of well-burnt lime, and pour water on it till it falls to powder. I then mix it with a further quantity of water, in order to bring it into such a state that the particles of lime may adhere slightly to each other, but not to such a degree as to prevent the free passage of the air between. This mixture must be placed six inches deep, more or less, on movable perforated shelves, in a vessel the top of which is guarded by a water-joint, and underneath is a pipe, to allow the passage of the gas that way, so as that the gas may pass from the bottom of the vessel to the top through the perforated shelves and lime mixture, or from the top to the bottom, as may be found most convenient, the purification being effected by the gas being caused to pass through the layers of lime mixture. But where the quantity of gas to be purified is very large I arrange a set of these vessels, consisting of five, or nine, or more, according to the size of the gasworks, each vessel containing one or more shelves."

This process was soon adopted by gas companies in all the large towns, and it remains to this day the process by which nearly all the gas of the world is purified, except in London and a few of the very largest towns and cities. Phillips patented his discovery in order to obtain remuneration, but the invention is said to have cost the unfortunate patentee his life. He thought, and thought reasonably, that he had a right to be paid for his invention by those who availed themselves of it to save expense and avoid prosecution for nuisance, and consequently he proposed that all companies which used his process should pay him a royalty for its use. But the companies, in a spirit of cupidity which will ever disgrace them, used the power of the purse to put down and defraud a poor but deserving man, whose invention was rendering them a great and essential service. I am informed that they used the process, then combined together and subscribed large sums of money to resist Phillips's claim. As soon as he attempted to enforce the claim by law, he was met by their formidable combination, and, worn out by the harass of legal proceedings, he died. His widow then abandoned the proceedings, and accepted any small sum which the gas companies were willing to give her for doing so. These facts are not within my own knowledge. My informant was one of those who, on behalf of the gas company in which he was a principal shareholder and director, entered into the unjust combination with the other companies.

It was not till 1849 that Hills introduced his process of using revivified oxide of iron for removing sulphuretted hydrogen from gas, but in 1818 a person named Palmer was not far from it. He proposed to purify gas by passing it through red-hot iron. This process could not be made effectual, as a large quantity of the light-giving matter of the gas was destroyed by its passage through the red-hot iron, while but little purification was effected. I mention him because he seems almost to have got at the notion of revivification of oxide of iron. He tells us that when one of his purifiers is thrown out of connexion, it does not need to have its contents removed, but they can be revived by the action of the air.

He describes his process thus: "The purifier is to be kept moderately red-hot, and is to be filled with fragments or refuse clippings of sheet iron, or any oxide of iron, at a minimum of oxidation, such as common clay or argillaceous iron ore, or finery cinders, or black oxide of iron; and when so filled and treated, the gas must pass through it, which will effect a partial decomposition of the sulphuretted hydrogen, and will be completely effected by passing into a box or cistern of cold water. *When one of the purifiers is thrown out of action it need not have its contents removed, but its doors must be left open to expose it to the action of the atmosphere, and as it is still kept red-hot, it will, before the second puri-*

fier becomes saturated, be again competent to purify the gas." If Palmer had changed the method of his purification to a cold process instead of a hot one, and had exposed his foul material to the air, he would have anticipated Hills; for having got so far, the phenomena with which he would have become familiar must have led him the rest of the way to purification by revivified oxide of iron.

In 1840, Croll proposed the use of oxides of manganese, zinc, and iron for purifying gas from sulphuretted hydrogen, and, so far, he was but doing what had been done before. But he went further, and proposed a method by which they might be renewed after they had become foul, and might be used a second time, if not more than twice. He describes his process thus: After the gas has been freed from ammonia, it may be further freed from sulphuretted hydrogen by passing it through black oxide of manganese in powder, moistened with water to about the same consistency as the lime usually employed in the dry process. The oxide above named may be revived and used again by heating it red-hot in an oven, and stirring and roasting it for some hours. The same effect may be produced by the application of the oxide of zinc and the oxides of iron, treated in precisely the same way as above described. In the protracted legal proceedings which ended in completely establishing Hills's claim to be the inventor of purification of gas by revivified oxide of iron, an attempt was made to show that this specification of Croll anticipated Hills. That he not only mentioned oxides of iron as purifiers of gas from sulphuretted hydrogen, as others had done before him, but that he revivified foul oxide by heating it in an oven. This attempt fortunately failed, and Mr. Croll's own evidence, as well as the process he specified, proved distinctly that he had no notion of hydrated sesquioxide of iron as a purifier, and did not intend to, and did not, in fact, produce hydrated sesquioxide of iron fit for purifying gas economically. He meant to drive off the sulphur by heat, and the result of his process was the production of anhydrous sesquioxide of iron, which will, no doubt, in a measure, take sulphuretted hydrogen from gas, but which would not be useful to purify gas commercially.

After all, Croll's process is not original; it is but a republication of Palmer's, and may have been borrowed from him. Not only was Palmer's specification published by being lodged in the Patent Office, but his process was well known among the better class of managers of gasworks, of whom Mr. Croll was one. Palmer's process was mentioned by Mr. Brande in his Bakerian lecture on coal gas in 1820, as well as in other books, which were probably well known amongst persons engaged in the manufacture of gas.

The name of Malam is honourably associated with the progress of gas-lighting, but it is also associated in one particular instance with a number of absurdities which I mention as a warning to those who may feel inclined to meddle in chemical matters without the previous labour which all must undergo who wish to succeed. In the year 1844, Mr. Malam patented "Improvements in purifying coal gas and increasing its illuminating power, and preventing the circulation of it being impeded by frost." Part of his improvements consisted in freeing coal gas from aqueous vapour. He did this by passing it through a series of condensing or precipitating vessels, called freezing cylinders. These freezing vessels contained agitators, and the revolution of these agitators caused the gas to come into contact with the cold surface of the cylinders, and to deposit its moisture in the shape of "icy particles resembling hoar-frost."

After the gas had been subjected to this process, he still further desiccated it by causing it to pass through another set of cylinders, where it was "exposed to the absorbing action of alcohol or any other strong spirit."

The effect of freezing would be to condense the greater part of the hydrocarbon vapours, and thus render the gas nearly lightless, and the small residue of illuminating matter (if any) left from the freezing would be taken out by the alcohol, and the gas would have very little, if any, greater illuminating value than a mixture of carbonic oxide and marsh gas. Nor is this all. The cost of the process would have been enormous. The Spartans used to exhibit their slaves drunk to warn their children against drunkenness, and this instance may well be used by many persons in a similar manner, to caution them against falling into chemical error.

About this date the use of gas was increasing very rapidly, and gas companies in London and in some large towns were sadly encumbered with and troubled about the lime refuse which arose from purifying their gas. Purified the gas must be. Sanitary regulations most properly prevented the running of Blue Billy into streams and rivers, and even in some cases prohibited the carting away from gasworks of dry lime refuse during the daytime. Matters seemed to be approaching a crisis, when, in 1849, Mr. Hills came to the rescue and introduced hydrated sesquioxide of iron as a purifying agent instead of lime. He showed that when the oxide had become foul so as to be no longer useful, it might be restored to even more than its original efficiency by exposure to the air, which removed the sulphur from the iron, with which it was in combination in the purifiers, and set free the oxide for renewed use. He showed, further, that this power of the air to restore the oxide was exerted every time the foul oxide was exposed, and that, so far as the revivification by exposure to air was concerned, one supply of oxide would last for ever. A practical limit to its use, however, was put by the clogging of the material by precipitated sulphur. When a certain quantity of sulphur has been set free in a mass of oxide of iron, it becomes useless, not because its chemical affinity for sulphuretted hydrogen is destroyed or even lessened, nor because the air has lost the power to restore it when it is foul, but because it is clogged with inert sulphur. Mr. Hills further showed that when thus useless as a purifying agent the oxide yet had value, for the accumulated sulphur could be converted into sulphuric acid; and for this purpose the oxide was worth what it cost at first, in some cases, I believe, even more. This was an enormous boon to urban gas companies. It delivered them from a great difficulty in reference to foul lime, and it taught them how to free their gas from sulphuretted hydrogen without any cost for material to do so. Remuneration to the patentee during the currency of his patent was all which this advantage could cost them. Several companies arranged with Mr. Hills for licenses, and used the process during nearly all the time of the patent; but a considerable number entered into a compact to defeat the patent if possible, and at any cost, and the lawyers hardly ever had such pickings as from the cases of Hills *v.* some one or other. In his evidence given, when he unreasonably asked, but was properly refused, a prolongation of his patent, Mr. Hills stated that his law expenses had amounted to a prodigious sum. It is well known that Mr. Hills finally established his patent and obtained a large remuneration, which the majority of lawyers and chemists think he was most justly entitled to. Nearly all impartial persons, who are not connected by interest or feeling with either side in the controversy, are of opinion that right was with Mr. Hills, and that the combination against him was entirely unjustifiable.

Although this process of purification is a great boon to large gas companies, it has its drawbacks for consumers. First, because since it came into use much carbonic acid is left in gas, and thus its illuminating power is reduced; and secondly, because when the foul oxide is spread out to revivify, it gives off a quantity of very offensive compounds, which seriously annoy those who reside in the neighbourhood of a works where foul oxide is exposed to revivify in the free air, instead of in covered and nearly close vessels or chambers. These drawbacks are quite unnecessary. The use of lime after the oxide would remove the carbonic acid from gas, and need not produce any nuisance, or involve more than a merely nominal cost, as the lime might be made fit for use for the removal of carbonic acid over and over again. The evolution of the sickening vapours emitted by foul oxide need not be a source of annoyance, as the oxide can be revivified in the purifiers themselves, or in a building, whence all effluvia can be carried through fires into the atmosphere so as to be harmless. The cause of the effluvia will be understood by considering the action of air upon the foul oxide. When the mass of foul oxide and sawdust is removed from the purifiers, it contains water, sulphide or sesquisulphide of iron, gas and hydrocarbon vapours. The air acting upon this mass produces a quantity of water and much heat, indeed, where air has been forced into foul oxide in purifiers, the mass has become in some instances red-hot from the intense chemical action. The effect of the heat is to drive off volatile matters, and leave the oxide fit for use again. The gas retained mechanically in the mass is soon dissipated, and being less than half the specific gravity of the atmosphere, ascends and mingles with the upper air rapidly. The aqueous vapour and the vapours of hydrocarbons are much heavier than the atmosphere, and when evolved from the foul oxide by the heat of chemical action, they float in the lower stratum of the atmosphere near the surface of the earth, and produce extensive annoyance. There are few smells more offensive to many persons than the smell of these compounds given off by the oxide in the process of revivification, and in every gasworks they ought to be consumed instead of being allowed to escape. If this be not done voluntarily, the law ought to compel it. In answer to this, it will be urged that it was tried in one case, and led to an explosion which blew down a chimney in a gasworks, *and therefore it cannot be done without great risk.* I reply to this, that a mistake such as the one referred to proves nothing, except the carelessness or ignorance of those who caused the accident. I can point out a way of burning the compounds evolved from foul oxide, which shall involve no more danger or difficulty than the removal of dirt from a carpet by a broom, and when a nuisance is preventable with a little care and expense, no one ought to be allowed to continue the nuisance to annoy or injure neighbours. Good feeling should lead to an abatement of the nuisance without a mention or even a thought of the law.

The spent oxide of iron in every case consists of oxide of iron and sulphur principally; but the other constituents of the refuse vary considerably—first, on account of the differences in the oxide employed; and, secondly, on account of the coal employed to make the gas purified, and the manner of its distillation. No single analysis can do more than give a general idea of this refuse, and for this purpose I append one published by Dr. Phipson.

" When this gas refuse has been exposed to the air for some time, it contains the following substances: free sulphur in considerable quantity, oxide of iron, carbonate of lime, cellulose in small quantity, some hydrocarbons soluble in alcohol, double cyanide of iron (green), ferrocyanide of iron (blue), sulphocyanide of calcium, sulphocyanide of ammonium, chloride of ammonium, sulphate of lime, ferrocyanhydric acid (to which the mass owes its acid reaction), and water. Some of these are present in small quantity only; but it is not difficult to put them all in evidence. A rough analysis of the whole has given me—

Water	14·0
Sulphur	60·0
Organic matters insoluble in alcohol	3·0
Organic matters soluble in alcohol, sulphocyanide of calcium, chloride of ammonium, hydrocarbons, &c.	1·5
Clay and sand	8·0
Carbonate of lime, oxide of iron	13·5
	100·0

" Hot water extracts sulphocyanide of calcium, and ammonium, sulphate of lime, and ferrocyanhydric acid. The solution takes a red colour with persalts of iron.

"Hydrochloric acid dissolves a considerable amount of the substance, and the solution obtained is deep blood-red, almost opaque, from the presence of sulphocyanide of iron formed.

"Alcohol extracts principally sulphocyanides of calcium and ammonium, chloride of ammonium, a small quantity of hydrocarbons, and ferrocyanhydric acid.

"By evaporating the aqueous solution to dryness, after having added enough carbonate of potash to neutralise its acidity, and treating the residue by alcohol, the ferrocyanhydric acid is left behind as ferrocyanide of potassium, and the alcoholic solution contains only chloride of ammonium and sulphocyanides of calcium and ammonium. By evaporating in presence of an excess of carbonate of potash to complete dryness, and treating with alcohol, the latter takes up principally sulphocyanide of potassium; and by using carbonate of ammonia, in place of carbonate of potash, the alcoholic solution consists of sulphocyanide and chloride of ammonium, which would serve for fixing photographic proofs.

"The green compound which forms when gas refuse is exposed to the air is no other than the double cyanide of iron, $Fe\ Cy + Fe_2\ Cy_3$, discovered by Pelouze, the composition of which corresponds to magnetic oxide. By prolonged oxidation in the air it becomes blue; cold acids have no action on it, but hot nitric acid decomposes it."—*Chemical News*, vol. vii., p. 170.

This green compound has been familiar to me for the last ten years. It may be obtained in considerable quantity from clay which has been used to purify gas, and it is from that source I have obtained it.

In 1854, I introduced the use of clay as a purifier, first and principally to remove ammonia without damaging the light-giving constituents of gas, and secondly to remove sulphur compounds, which other processes failed to effect. Clay removes ammonia from gas as perfectly as acids and metallic salts do, and if its action were confined to the mere removal of ammonia it would not be entitled to any preference. On the contrary, acids would be preferable to it. But acids cannot be used without a considerable destruction of the illuminating constituents of gas, whereas the use of clay does not destroy any of those constituents, but, on the contrary, renders gas more illuminating by the removal of impurities which reduce the illuminating power of gas if left in it. On this ground, if on no other, it is entitled to preference. But it has a remarkable power of acting upon certain sulphuretted compounds in gas so as to render them removable, and thus its claims to adoption become strong. If gas supplied to a house, free from sulphuretted hydrogen and other compounds which blacken lead-paper, be passed through a series of vessels containing clay and lime alternately, it is found that, after passing through clay, the gas will blacken lead-paper, owing to the action of the clay upon certain of its sulphuretted constituents, whereas upon subsequently passing through lime the sulphur compound which blackened the test-paper is removed, and the test-paper is unaffected. This result is obtained upon passing gas through a series of ten or twelve alternate vessels. The beneficial influence of this power of clay must be obvious, and till I discovered the power of heated lime to free gas from sulphur there was nothing which could compete with clay as an auxiliary to lime. By alternating clay and lime in the purifiers of a gasworks, I have seen gas made in *iron retorts* so far purified from sulphur, that half a pint of water condensed from its combustion gave but a faint opalescence when a salt of baryta was added to it after acidification and heating. When gas is made in *clay retorts* at the high temperatures now generally employed, clay cannot remove so large a proportion of the sulphur, owing to the different chemical combinations in such gas as compared with those in gas made at lower temperatures in *iron* retorts. But even on this gas clay acts most beneficially, and reduces its quantity of sulphur considerably. By alternating it with lime it would reduce the quantity of sulphur in much gas within the maximum allowed by Parliament.

A purifier on the premises of the consumer will mitigate the evils of sulphuretted gas very much, for although it will not render the gas as free from sulphur as it ought to be, it will lessen the quantity materially. Even with the best and purest gas sold, such a purifier will be found useful. The lime will become highly sulphuretted, and the clay will evolve sulphuretted hydrogen when treated with a mineral acid, and ammonia when treated with a fixed alkali.

Purification of gas by clay entails no cost upon gas companies which use it; but, on the contrary, produces a considerable revenue. I can now speak after eleven years' experience, and the use of the foul clay for manure over tens of thousands of acres, upon nearly all the crops grown by British farmers, and can say that nothing which the farmer buys can compete, money for money, with the foul clay which has purified gas. The best proof of its efficacy and cheapness is found in the fact that those who used it sparingly at first in mere experiments have continued to use it year after year in increasing quantities, and have borne frequent testimony to its value, especially for grass and cereals.

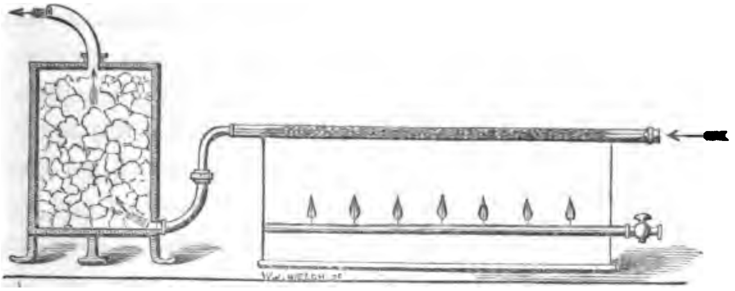
This manure is sold at twenty-one shillings per ton, and its cost to a gas company is but little. Many companies have clay upon their own premises, and these could supply themselves at a merely nominal cost. Where clay is purchased, it may generally be obtained at from sixpence to two shillings a cartload, weighing about twenty-four hundredweight, and this after having purified the gas, and thus done a positive good, is worth a guinea per ton. The quantity of manure which can be made varies somewhat with the coal used, but seven tons to each million feet of gas is about an average. Care must be taken to decompose the clay thoroughly, by exposing it to the *crude gas* the last time it is used. This can be done with advantage to the gas as well as to the manure. The foul clay pulverised for manure may be stored for a whole season, as it retains its virtues when kept nearly as well as guano does.

In 1860, I made known the fact that the action of lime upon bisulphide of carbon is different at different temperatures, and pointed out a practical method by which gas may be freed from sulphur. At common temperatures, hydrate of lime forms with bisulphide of carbon a very beautiful ruby-coloured crystalline salt. Between 110° Fahr. and about 700° Fahr. it converts bisulphide of carbon into other sulphuretted compounds which cold hydrate of lime and alkalis take up, but to which the heated hydrate is perfectly indifferent. Though it converts the one compound into the others, it does not unite with the new compounds after they are formed. In fact, the properties of the lime are so changed, that a current of sulphuretted hydrogen may be sent over it for a great length of time without more than a trace being taken up, whereas at common temperatures the affinity of slaked lime for this gas is known to be very powerful. This property of lime is not lost by the heating, but only suspended; for if it be allowed to cool and be moistened, it will absorb sulphuretted hydrogen as readily as ever. Lime must contain more water than will form a simple hydrate, $CaO\ HO$, before it will absorb sulphuretted hydrogen with energy. At a red heat the hydrate of lime acts differently. It then unites with sulphuretted hydrogen, and forms sulphide of calcium. It also forms sulphide of calcium with the sulphur of bisulphide of carbon. My process of purification consists of passing gas first through heated hydrate of lime, by which the sulphur of its bisulphide of carbon and sulphuretted hydrocarbons is so altered as to be removable; and then, secondly, of passing the gas containing these altered compounds through ordinary "dry lime," which removes them from the gas. Gas may be thus rendered absolutely free from sulphur; or it may be so far purified, that five or six lights burning all night in a room will not give off more sulphurous acid than is produced by the sulphur on the lucifer-match by which the gas is lighted; or part of the gas of a company may be thus purified and mingled with the rest, so as to keep the sulphur always below the maximum allowed by Act of Parliament. It is hardly necessary to say that I am an advocate of the more perfect purification.

Cost of the Process.—The cost of this process is very small, for the heated lime possesses the power of converting the irremovable sulphur compounds of gas into removable ones during many months' continuous use. One friend of mine informs me that he has had a tube of hot lime in his laboratory in use for fourteen months, and that its action at the end of that long period was very effective. This shows that the quantity of hot lime and the labour of changing the purifiers will be small. In another case, an experience of more than two years shows that the purification is efficient when the purifiers are changed once in three or four months. The layer of heated lime through which gas is passed need not be above two feet in length, so that the quantity required is thus seen to be small. The best general working temperature is between 300° and 400° Fahr., a temperature which might easily be maintained in a gasworks by the spare heat from the retorts or steam engines, and which in that case would cost nothing. The quantity of cold lime required will depend upon the amount of sulphur in the gas, and the quantity of that sulphur which it is determined to remove; but as the sulphur will not often exceed twenty grains per thousand feet of gas, it is easy to see that the cost for cold lime cannot be much. Some engineers in towns, especially in London, are frightened at the thought of having to remove lime which has purified gas. Their fears arise from experience relative to ordinary gas lime, which evolves most offensive compounds upon exposure to the atmosphere. But no such fears need be entertained relative to the offensiveness of the spent lime which follows the hot lime in use. The compounds it contains are mostly different from those contained in ordinary gas lime, and a purifier of it would give off less offensive vapours than the same sized purifier of foul oxide of iron. It has been asserted that the gas, after passing through the hot lime, will require condensation—an assertion which must have had conjecture for its parent, as it could not spring from fact. The difficulty of heating gases and vapours is well known, and the bad conducting power of lime is also well known, and these are the materials acting and acted upon in this case. Gas passed though two feet of lime heated to 300° Fahr. will not be warmer than the gas of a large London gasworks used to be when it passed the purifiers in the time of its late engineer, and without another word, this fact is conclusive against the speculative objection I have mentioned.

A yet more serious objection has been made, viz. that passage of gas through hot lime robs it of much light. If this happen at all, it must happen in one of two ways: 1, by decomposition of part of the gas, and deposition of its carbon upon the hot lime; or, 2, by decomposition of condensed luminiferous constituents, and the formation of more bulky and less illuminating constituents. If the former of these things happened, the hot lime would soon become covered with deposited carbon, and so inactive. That this does not happen is proved by the activity of hot lime after fourteen months' use in one case, and three months' use in another; and by the solution of lime after use in hydrochloric acid, which would leave the carbon as an insoluble residue which can be filtered off from the chloride of calcium and weighed. After a month's use, but a few millegrammes of carbon can be thus obtained from a tube full of lime. In the second case, the increase of bulk would be a gain to the gas company, which would hardly be a cause of offence. But this accusation has been fully investigated and as fully disproved. The chairman of a gas company made this charge against the process, and the gentleman to whom it was made, gas-analyst in a neighbouring town, informed me thereof. I at once demanded an investigation to be made by an impartial and competent person, in the presence of the two gentlemen I have mentioned and myself, and any number of other persons of competency they might bring. It was made in the presence of six persons—viz. myself, my assistant, the person to whom the charge was made, the person who made the charge, and the official gas-tester and secretary of the gas company, of which he was chairman. The experiments occupied a whole day, from nine o'clock A.M. to seven P.M., and they were made alternately upon the gas as supplied to the town and the same gas after it had passed through hot and cold lime and thence to the burner. The result of the whole series of experiments was a gain of light in favour of the gas purified by hot lime of ·65, or $\frac{13}{20}$ths of a candle. Every reading of meters, pressure gauge, photometer, and thermometers, and every weighing of the candle, was examined by every one present, and the results were agreed to unanimously. Of course the statement objected to was withdrawn, and it was admitted that no loss of light arose from the process. This is a complete answer to the charge; and though I do not publish the names of those who were present, I am prepared to do so if it should ever be of importance, and to produce the original entries of the observations made at the time in the handwriting of the person who made them. I lay no stress upon the slight gain of light arising from the process, although it agrees with the results obtained by Dr. Sheridan Muspratt with the gas of Liverpool. It is enough that it does not injure gas, and, where it is very impure, that it benefits it slightly by the removal of impurity. Should any one assert, therefore, that sulphur cannot be removed from gas, or that the process for doing so is costly, or that it injures the light-giving powers of the gas, he may be told at once that either assertion is demonstrably untrue. I am prepared to demonstrate the untruth of all these assertions at any time when the experiment can be made under such circumstances and supervision that fraud and error are impossible, and the results can be published, attested by the signatures of those under whose supervision they are obtained.

I cannot refrain from mentioning an amusing *practical* test to which my process was put by a gentleman in the neighbourhood of London, who has had it in use some time to purify the gas supplied to his house. When he purified his gas, he himself and his family experienced much less annoyance from the products of combustion than they had been accustomed to do; and his wife was able to sit with the doors of the rooms closed, which she was unable to do previously. Being determined to know whether his wife's feelings sprang from fact or from fancy, he took the management of the purifying apparatus out of the hands of the servant who managed it into his own. For a week or two all went on as usual, but at length he put the purifying apparatus out of use without the privity of any one. Upon returning home in the evening, his wife had the drawing-room door open, and complained much of the gas; and for several days, while the purifier was out of action, she did the same. He then put his domestic apparatus into action, and purified the gas, without saying a word to any one, and the complaints ceased directly. He repeated this experiment three or four times, till he was convinced that the lady's uncomfortable feelings were the result of impurities in the gas which the hot lime purification removed. He says that he is able to tell by the atmosphere of his rooms at night when the apparatus requires fresh lime, and most of his family make the same observation as himself.



Purifiers for domestic use are made and sold as articles of commerce, so that any one who pleases may remove sulphur from the gas he burns as easily as he filters mechanical dirt from his water by his domestic filter.

Water has been used to purify gas ever since the time when Winsor "washed the smoke," and it is a great favourite with some engineers. Years ago, most gas-works had "washers," and even yet many may be found where considerable quantities of water are consumed annually in washing (I nearly wrote, spoiling) the gas. No greater mistake can be made than this. Water cannot be used to dissolve the impurities of gas without at the same time dissolving its most valuable illuminating constituents; and whenever it is used in sufficient quantity to remove what is hurtful, it reduces the illuminating value very much. This is a matter of considerable practical importance at the present time, for washing has come again into fashion, and is strongly recommended. In spite of fashion, I take this opportunity of repeating what I have said and written often—that water is the most damaging substance which can be used in purifying gas, and that its use is a deliberate wasting of the illuminating matters which it is the object of a gas engineer to produce, to conserve, and to deliver to his customers. Valuable illuminants are produced, and then washed away and rendered useless. Any engineer may satisfy himself of this by determining the illuminating power of his gas, and then passing a few feet of it slowly through a large cask of water into a test gasholder. The illuminating power of this washed gas will convince him of all that can be said on the hurtful influence of water upon gas.

It is right to say that Dr. Letheby has recently told a meeting of gas managers the very opposite of this. He is reported by the *Chemical News** (and I believe he furnished the statement himself) as saying: " *It is a fact that water has little or no influence on any of the hydrocarbons except acetylene;* and, as this exists in gas only to a very small extent, there is little or no danger from a copious washing of the gas before it goes to the purifiers." This statement is evidently made deliberately, for in a table on the next page a column is devoted to the solubility of the constituents of coal gas in water, wherein all below benzole are represented as absolutely insoluble in water, and benzole as soluble only to the extent of ·8 per cent. Except ethylene and acetylene, all the others are represented as practically insoluble. The error of this admits of easy proof. I have had considerable experience in the purification of the heavy hydrocarbons, cumole, cymole, naphthalin, &c., and have seen water used to wash them hold in solution or suspension (for I am not disposed to cavil about terms) upwards of 7 per cent. of these substances. The hydrocarbons were so mixed with the water as to be inseparable mechanically. They would filter through in solution as would common salt in water, and remain for days (sometimes for weeks) in solution before they separated. At length the attractive force of their particles for each other overcame the force by which they were separated in the water, and the greater portion separated on or under the water, according to their density. Now the reader must remember that coal gas owes its illuminating value chiefly to the *vapours* of hydrocarbons, and that a cubic foot of *vapour* condenses into an exceedingly small bulk of *fluid* or solid. Then, if water could take up and retain in an inseparable condition for days, and even for weeks, as much as 7 per cent. of these substances measured in a *fluid* condition, it does not require a great stretch of imagination to believe in its power to remove from gas, and retain long enough to rob the gas of a serious amount of luminosity, much of the comparatively small quantity of hydrocarbon vapour to which it owes most of its usefulness as a light-giving agent. What carries down from stored gas the many tons of fluid hydrocarbons to be found floating on and dissolved in the water of gasholder tanks? Condensed aqueous vapour. What carries down such considerable quantities as are found in the gas mains? Condensed aqueous vapour. I have gallons of these hydrocarbons in my laboratory which were sent to me, mixed with water, as they were pumped from the mains, and have been since separated. If any chemist will separate a few gallons of these hydrocarbon oils from water, and will obtain the solid substances dissolved in the water (especially if the oils have been deposited by cannel gas), he will receive a lesson upon this subject which he cannot forget.

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* Vol. xii. p. 16.

Mr. Anderson was good enough to send me, quite recently, a published paper of his, wherein this statement of Dr. Letheby is referred to and confuted. He writes: "It is within my memory that more than a year ago I stated that 13-candle gas allowed to bubble through a column of water of moderate height suffered deterioration to the extent that it became less than 8 candles of illuminating value. I have caused the following experiment to be made, as further proving this point. A gasholder was filled with gas, and a small fountain of water was caused to play amidst the gas, by which the gas was exposed to the action of a very finely divided rain. The deterioration of the illuminating value after half an hour's play of the fountain was nearly 33 per cent." Part of this reduction of illuminating value was due, I think, to air given up by the water to the gas; but when all proper allowance has been made for this, there remains sufficient to convince an impartial person that the damage done to gas by water is considerable.

It is no answer to this to allege the washing of gas by ammoniacal liquor which has been condensed from gas, because this liquor always contains a considerable quantity of hydrocarbons—perhaps is always saturated or nearly saturated with them—and is therefore unable to abstract from gas any, or, at all events, much more. While these sheets were being printed, the annual meeting of the British Association of Gas Managers was held in St. Martin's Hall, London. At that meeting the president, Mr. Hawksley, delivered an address, wherein he makes a statement relative to the power of water to dissolve illuminating matter from gas, which looks almost like an intentional answer to what Dr. Letheby told the same meeting last year. He is reported by the *Chemical News** as saying: "There is this disadvantage, however, in the use of pure water—*it takes out the illuminating power of a gas, so that an* 18-*candle gas is brought down to* 3 *or* 4 *candles.*" He represents this as the result of experiments made under his own superintendence at the Nottingham Gasworks, of which he is the engineer. If Dr. Letheby's statement be reliable, this of Mr. Hawksley is a physical impossibility, and is simply incredible. One says: "Water takes out the illuminating power of a gas, so that an 18-candle gas is brought down to three or four candles;" the other says: "It is a fact that water has little or no influence on any of the hydrocarbons except acetylene; and as this exists in gas only to a very small extent, there is little or no danger from a copious washing of the gas before it goes to the purifier." Mr. Hawksley's loss by water is greater than I have ever found, and no doubt gases are so differently constituted that water will affect them differently; but without hesitation I should receive his statement in preference to the one that water will not injure gas so much as to render it a dangerous substance for use in purification. In Clegg's book on coal gas, Dr. Frankland makes a statement which agrees with general experience on this question. He says: "Water possesses the property of absorbing or dissolving gases, and a knowledge of its relative solvent action upon the various gases is of great importance to the gas manufacturer. One hundred measures of water at 60° Fahr. can absorb 12·5 measures of olefiant gas, *and probably more than* 12·5 *measures of illuminating hydrocarbons.*" I do not think another word is requisite to caution engineers against the free use of water in the purification of their gas.

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* Vol. xiii. p. 261.

There is a branch of the washing of gas which demands just a passing notice. I refer to the oft-repeated statement that washing with ammoniacal liquor in addition to ordinary purification frees gas from sulphur. From the manner in which this is often put, we might suppose that washing gas with ammoniacal liquor was a novelty, and that its effect had just been discovered, instead of the washing being upwards of twenty years old, and practised upon a large scale by the very companies which cannot keep within the large limit of 20 grains of sulphur per thousand feet of gas. Mr. Lowe has told us how long ago, and for how long, he washed gas with gas liquor, and that he could not free it from sulphur. Mr. Mann has invented and used a most efficient apparatus for this purpose, and he is a stranger to the boasted result, and others who are doing, and have long done, this very thing, join in testifying that they know of no such result as freeing gas from sulphur thus, though they are acquainted with the pecuniary gain obtained by the washing. The gain is a fact, but the freeing gas from sulphur thus is as mythical as the labours of Hercules. Mr. Anderson was good enough to send me his pamphlet on "The Action of Sulphides of Ammonium upon the Bisulphide of Carbon and Sulphuretted Hydrocarbons in Gas," and even if his laboratory results be confirmed by other investigations they would not bear upon the question of mere purification by gas liquor. Unless I mistake, there is an unsuspected source of error in his experiments which it would not be difficult to point out at the proper time and place. At all events, the one complete answer to the benefit of scrubbing with ammoniacal liquor is found in the quantity of sulphur contained in the gas supplied to the City of London.

GAS REFUSE AS MANURE.—Having had considerable experience in this matter, I may, perhaps, add a few words upon it in completion of the subject.

Gas lime has been used very largely as manure, and the agricultural estimate of its value may be learnt from the fact that in large towns it cannot be given away, while in the strictly rural districts it fetches from about a third to a fifth of the price of lime. When applied in too large quantity, gas lime does considerable injury; in fact, it is capable of destroying all vegetation for several years, if applied freely enough. Hence its use under garden paths, and in other situations where prevention of the growth of weeds is desired. But there are certain cases where benefit has arisen from the use of gas lime, and where its judicious employment has yielded a profit. I have met with cases of red soils where an application of 5 or 6 cwt. an acre has been found to benefit corn.

Such cases are easily explained. Soluble sulphides would be decomposed by the oxide of iron of the soil, and would yield harmless sulphur when oxidised by the air, instead of hurtful compounds, such as would come into contact with the roots of plants in the absence of the oxide of iron, or when the refuse lime was applied in excessive quantity. The truth is, gas lime yields a large proportion of soluble lime salts, and hence requires to be applied with judgment. If 4 or 5 cwt. an acre were applied every other year, I do not think that the application could be otherwise than beneficial. Mixing gas lime with compost is worse than useless. The soluble lime salts are not converted into insoluble ones in an entire twelvemonth, and any little ammonia which may happen to be formed in the compost heap is set free by the caustic lime which always exists in gas lime in large proportion. I once saw gas lime mixed with fold dung in a field. The evolved ammonia could be smelt above a hundred yards from the heap, and a fine tree near it was entirely destroyed.

SPENT OXIDE.—This is now used for the manufacture of sulphuric acid, but it was tried as manure with most disastrous results. Where it was applied freely, it destroyed all existing vegetation, and nothing would grow upon the ground for years. At length the destructive power was itself destroyed, and the land resumed its fertility.

AMMONIACAL LIQUOR.—It is not worth a farmer's while to buy this in large towns, owing to its value for making ammoniacal salts, and the large quantity of useless water which he is obliged to buy and carry to obtain the manurial compounds. It may be obtained from small country works, and from private works, where gas is made for domestic use, &c., and then it forms a good manure, especially for grass. It should be diluted with at least three times its own bulk of water before it is applied to the land. Should grass at any time be a little browned by the application, it soon recovers and grows vigorously.

FOUL CLAY.—I have already spoken of this, and would only add, that while nothing can compete with it, for efficiency and cheapness, when applied seasonably and with judgment, it is capable of doing harm if applied out of season or in excessive quantity. Those who sell it supply directions for its use, which have been drawn up from extensive experience, and which should be adhered to rigidly in order to obtain the full advantage which the manure is capable of yielding.

CHAPTER III.

LIGHT-MEASURING.

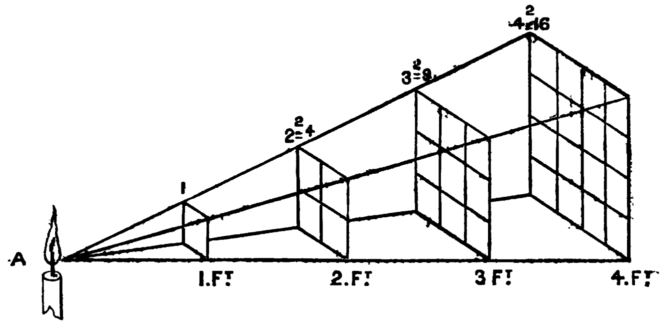
A PHOTOMETER is a light-measure, and its indications tell us the quantity of light given by a luminous body submitted to its test. We are no more able to judge correctly by our senses how much light is given by a luminous body, than we are to determine how much matter is contained in a solid; and if we really intend to pass beyond the merest guesswork, we must weigh the one and measure the other. In the early days of gas manufacture, the quantity of light given by a known quantity of gas was determined by a comparison of its shadow with the shadow cast by a candle, and the illuminating value of the gas was expressed in terms of the candle: thus, if five cubic feet of gas burnt in an hour gave a shadow ten times the depth of the candle employed, it was called ten-candle gas, and so on. But it is no more possible to say how much one shadow exceeds another in depth than it is possible to say how much one gas-light exceeds another by mere inspection. Wherefore recourse was had to a well-known law in optics, viz. that light varies inversely as the square of the distance; *i.e.* that as a body is removed from a light it receives less and less illumination, according to the distances to which it is removed, and that if at any two distances we fix the body and square the distances, the illumination the body receives is inversely as those squares. For instance, let the light which the illuminated body receives at one foot distance from the light be called one, and let the first distance to which it is removed be two feet, and the second three feet; then, by squaring those distances, we have an inverse measure of the illumination received by the body from the light. $2^2 = 4$ and $3^2 = 9$, wherefore the light received at three feet distance is $\frac{1}{9}$ that received at one foot, and that received at two feet distance is $\frac{1}{4}$ that received at one foot. If we remove the body one foot farther from the light, and square its distance, viz. four feet, we see that its illumination is but $\frac{1}{16}$ that which it received at a foot distance. This is shown in the following series:

Distance from light . .	1 foot.	2 feet.	3 feet.	4 feet.	5 feet.
Amount of light received . .	1	$\frac{1}{2^2} = \frac{1}{4}$	$\frac{1}{3^2} = \frac{1}{9}$	$\frac{1}{4^2} = \frac{1}{16}$	$\frac{1}{5^2} = \frac{1}{25}$ &c.

Inverting these, we have:

Light received at 1 foot = 25 times the light received at 5 feet
,, ,, ,, = 16 ,, ,, ,, 4 ,,
,, ,, ,, = 9 ,, ,, ,, 3 ,,
,, ,, ,, = 4 ,, ,, ,, 2 ,,

People find great difficulty in apprehending the very rapid decrease of illumination caused by distance from the illuminant, and some good physical illustrations have been given to render its comprehension easier. Suppose a person placed in a perfectly dark room, and a light, A, to be in another room, and a hole a foot square to be cut in the door of the dark room at 1 ft. from the light, and a screen to be placed close to the hole in the door. A square, 12 in. on each side, would be illuminated by the light, and the rest of the screen would be dark. If the screen be moved a foot from the hole in the door, and therefore 2 ft. from the light, the illuminated area will be a square 2 ft. on each side, or 4 superficial feet. If the screen be placed 1 ft. farther off the door, *i.e.* at 2 ft. distance from the hole, and 3 ft. distance from the light, its illuminated area will be a square 36 inches on each side, and therefore containing 9 superficial feet. Similarly, if it be removed 1 ft. farther still, *i.e.* 3 ft. from the hole, and 4 ft. from the light, its illuminated area will be a square of 48 in. on the side, which will contain 16 superficial feet, and so on for every additional foot by which it is removed from the light. Now, the number of feet illuminated will be found exactly the same as the squares of the distances at which the screen is placed from the light.



$1^2 = 1$ = portion of screen illuminated at 1 foot distance
$2^2 = 4$ = „ „ „ 2 „
$3^2 = 9$ = „ „ „ 3 „
$4^2 = 16$ = „ „ „ 4 „

So that the area illuminated is found to vary directly as the square of the distance from the light. But how much light falls on a given unit of surface? *All* the light which passed through the hole fell upon the screen when it was close to the door, at 1 ft. distance from the light, and this is the entire quantity of light received by the screen at each of its positions. But the quantity which was concentrated upon 1 ft., at 1 ft. distance, was spread over 4 ft. at 2 ft. distance, and therefore the quantity upon each foot of screen must be a fourth of the entire light. Again, at 3 ft. distance the light spread over 9 ft. of the screen, and therefore the quantity upon each foot must be $\frac{1}{9}$ of the whole. So, at 4 ft. distance the light spreads over 16 ft., and therefore the light on each foot must be $\frac{1}{16}$th of the whole, and so on for any given distance. Wherefore, to find the amount of illumination of a given area, we have but to measure the distance of the illuminated body from the light, and to square that distance, and we have an expression of the quantity of light received by the given area, as compared with that which it would receive at a distance fixed upon as unity.

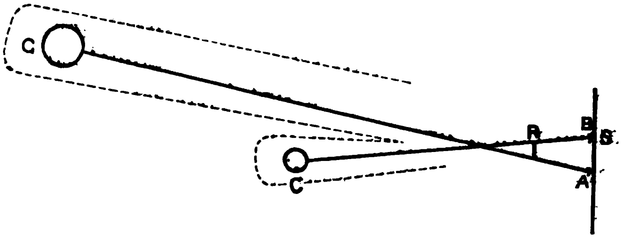
Again, suppose a light placed at the centre of a hollow sphere, 2 ft. in diameter, it will be 1 ft. distant from every point, and its light will fall upon the whole of the internal surface of the sphere, and will illuminate every part equally, for it is equidistant from every part. Place the light in the centre of a globe 4 ft. in diameter. It will then be 2 ft. distant from every point, and will illuminate every part equally. The surface of the second globe is four times greater than that of the former, because spheres are to each other as the squares of their radii. In this case, as $1^2 : 2^2$: or $1 : 4$: consequently each inch of the surface of the larger sphere will receive only one-fourth of the light which illuminated each inch of the surface of the smaller sphere; and yet the light is only twice as far from the surface of the larger as it is from the surface of the smaller. So, if the diameter of the sphere were 8 ft., the distance of the candle from the surface becomes 4 ft., *i.e.* four times as great as it was from the surface of the first sphere; and as the surface of the 8-ft. sphere is sixteen times as large as that of the 2-ft. one, each inch of the 8-ft. sphere would receive but $\frac{1}{16}$ of the light which illuminated each inch of the 2-ft. sphere. The reasoning applies to any increase in the sizes of the spheres, and in this illustration the same law is traceable as was seen in that of the screen.

An application of this law enables us to compare the illuminating power of two lights.

RUMFORD PHOTOMETER.

Count Rumford proposed to estimate light by means of what is commonly called the shadow photometer. His method was very generally used for several years, and an excellent method it is, when used by a careful and competent observer. It possesses the important advantage of being available anywhere without special apparatus. A sheet of paper, or a white tablecloth, a stick, and a rule or tape, can be obtained wherever the illuminating power of gas has to be determined, so that a photometer can be extemporised without much trouble. The usefulness of this method depends upon a delicate perception of colour and depth of shadow. A person who possesses this can use it easily, and obtain results accordant with those obtained by competent observers employing other methods, but if he do not possess this delicate perception the method is entirely useless to him. I have seen the most ludicrous mistakes made with it, and the person making the mistake has insisted that he was right when his error was pointed out; in fact, he was incapable of seeing the error. His perception was defective, not the instrument. This kind of thing is by no means uncommon. A distinguished mathematician of my acquaintance was requested to procure some blue ribbon for a lady. He fulfilled his commission, but the ribbon he brought was scarlet instead of blue, and when informed of the error, he persisted that the colour of what he had brought and of what was shown to him, which it was to match, was the same. Similarly many people, otherwise competent enough, cannot form even a remote guess at the value of the shadows in the Rumford photometer, and a very large class indeed are deficient in that educated perception requisite for its proper use.

The Rumford photometer consists of a white screen placed against a wall or other convenient support, and a rod of wood or metal placed a few inches in front of the screen, between it and the lights, so that they may throw upon the screen two shadows of the rod. G represents the gas-light, C the candle, R the rod, S the screen, A the shadow cast by the gas-light, and B the shadow cast by the candle-light. The lights should be screened by a dark box or by black curtains, shown by the dotted lines, and if the room be not black, some black cloth should be put upon the wall against



which the screen S is placed. The gas should be placed at a fixed distance, say 100 inches from the screen, and the candle be moved until the two shadows A and B are exactly equal. Thus, suppose the gas to burn 5 feet an hour, and the candle to burn 120 grains of sperm, and the shadows to be equal when the candle is 25 in. from the screen S: then the gas gives sixteen times the light of the candle, or is what is called 16-candle gas, for

$$100 \text{ in.} = 25 \times 4 \text{ and } 4^2 = 16, \text{ or } 25^2 : 1 :: 100^2 : x$$

$$\therefore\ 625\,x = 10000$$

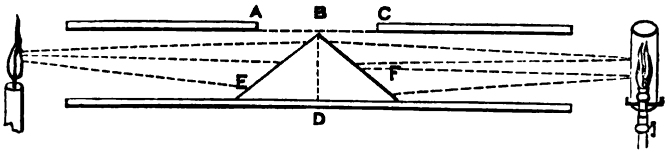
$$\therefore\ x = \frac{10000}{625}$$

$$= 16$$

or, divide the square of the greater distance by the square of the lesser distance, and the quotient is the value of the gas in candles. Or, if the candle be fixed at 10 in. from the screen, and the gas be movable, then by cutting off two figures from the square of the distance of the gas-light, we have the value of the gas in candles. Thus, gas shadow at 40 in. = candle shadow at 10 in. ∴ then $40^2 = 1600$ and $10^2 = 100$ ∴ $\frac{1600}{100} = 16$ candles: or 1600 with two figures cut off = 16.

RITCHIE'S PHOTOMETER.

The late Dr. Ritchie, of University College, London, constructed a photometer upon the principle said to be published by Bouguer in "Traité d'Optique," in 1760. I have used this instrument, and have found its results satisfactory, if it be employed carefully. His apparatus consists of a rectangular box, about an inch and a half or two inches square, open at both ends and blackened inside. Within are placed two sheets of plane looking glass, E F, cut from the same piece to secure equality of reflecting power. Each sheet has the same width as the box, and its length is equal to the hypothenuse of a right-angled isosceles triangle, whose side is the height of the box. Their reflecting surfaces are turned towards the open ends of the box, and their upper extremities rest against each other along a line, B, which divides the aperture, A C, into two equal parts, separated by a narrow slip of black card to prevent the mingling of the light reflected from the two planes. In using the instrument, it is placed between the lights which are to be compared, so that they may be reflected from B E and B F upon the tissue or oiled paper with which A C is covered. The photometer is moved towards the lesser light until the portions of paper, A B, B C, are equally illuminated. The distances of the lights from the vertical line, B D, are measured and squared, and their squares give the ratio of the lights to each other.



ARAGO'S PHOTOMETER.

M. Arago has proposed a method of determining the illuminating value of two lights which, in competent hands, may admit of very great accuracy, but which is unfit for use by any but good manipulators. His method is founded upon the properties of polarised light. When two lights are to be compared, the rays from each are polarised by passing them through a plate of tourmaline cut parallel to the axis, or by reflecting them from a plate of glass, on which they fall at the polarising angle. They are then received on a plate of rock-crystal, cut perpendicularly to the axis, and observed through a doubly refracting prism. Each light will thus give two images tinged with the complementary colours. The images are then brought into such a position that the red of the one falls over the green of the other. If the two lights be equal, this superposition will produce a white image; if unequal, the image will be slightly coloured with red or green, according as the one or the other light predominates. The apparatus which this method requires is somewhat complicated, and its manipulation must be attended with considerable trouble.—(See Notes to the French translation of Sir J. Herschel's "Treatise on Light," Paris, 1833. Brande's "Dictionary of Science," article, Photometry.)

BUNSEN'S PHOTOMETER.

The photometer generally used now is that suggested by Professor Bunsen, to whom science is so largely indebted. He saturates a piece of white paper with melted spermaceti and leaves a portion in the middle untouched. When the paper so treated is illuminated by a light placed on the same side of it as the observer, the untouched spot in the centre will look white, and the surrounding part which was saturated with spermaceti will look dark. In this case, the light by which the paper is seen is *reflected* light, *i.e.* light which passes from the luminous body to the paper, and is thrown back (reflected) thence to the eye of the observer. If the observer now place himself on the other side of the paper, things are reversed; the saturated part will appear light, and the centre spot will appear dark. In this case the paper is viewed by *transmitted* light, *i.e.* by light which has been sent through it; the portion of the paper saturated with spermaceti has acquired the property of translucency, and therefore allows light to pass through it and make it appear bright, when viewed on the side opposite to the light, whereas the spot of untouched paper in the centre retains its natural opacity, stops the light which falls upon it, prevents its passage through, and therefore appears dark to the spectator. But if we now place an equal light on each side of the paper, we find differences disappear, and the whole surface of the paper assumes the same appearance. Every portion is equally illuminated, and therefore every portion appears alike. Professor Bunsen employed this means to measure light. The prepared paper was placed between the two lights to be compared, and moved until it was illuminated equally on both sides, the equality of illumination being known by both sides of the paper appearing alike. The distance of each light from the paper was measured and squared, and the value of one was thus obtained in terms of the other. The following is a description of the instrument now generally used to measure light upon this principle.

The photometer consists of a bar of wood of about 98 in. in length, placed on edge, and having at each end means for fixing it to the candle-holder and gas pillar, so that the lights shall be exactly 100 in. apart. The wooden bar is graduated from the centre, which is the zero point, each way up to 36 candles. Towards the centre of the bar the divisions are large, and consequently they can be sub-divided into tenths; but towards the extremity of the scale they become too small to admit of sub-division, and each mark of the scale represents a whole candle. From 0 to 9 candles the scale indicates tenths, from 9 to 20 candles it indicates halves, from 20 to 36 candles it indicates whole candles.

A kind of saddle is made to fit the graduated rod so as to slide along it with very little friction, and to this saddle is fixed the prepared disc of paper held by metal rings. The centre of the disc is placed so as to be opposite the centre of the two lights, and the instrument is then ready for use.

To use the instrument, 5 ft. an hour of gas is burnt at one end through a 15-holed Argand burner which has a 7 in. glass chimney, and a sperm candle is burnt at the other end, which ought to consume 120 grains per hour, or as near that quantity as possible. The movable screen is moved gradually from the centre towards the candle until it comes into a place where both sides of the paper disc are equally illuminated, which is known by the spot in the centre becoming invisible, or, at least, nearly invisible, and equally distinct on both sides. The place of the disc on the graduated bar is then read in candles, by means of a small pointer fixed to the saddle, and this reading gives the illuminating power of the gas. To meet the changing light given by the gas and candle, a series of these observations is generally made, and the mean of the observations is taken as the illuminating power of the gas. A good plan is to make the testing last 20 minutes, and to take a series of ten observations, one every second minute. The mean of these, if carefully taken, fairly represents the light of the gas, as the time is long enough to allow of a fair appreciation of any fluctuations which are inseparable from a process with so many varying elements as photometry. When time can be spared, I prefer half an hour's test to 20 minutes.

Some persons may wish to graduate a photometer for themselves or to verify a purchased one, and I therefore append the method of calculating the divisions. Let x equal the distance from the candle of the point in the bar where the mark is to be made to represent a given number of candles, and let n equal that number, then the number of candles n, multiplied by x^2, is equal to $(100 - x)^2$ or

$$n\,x^2 = (100 - x)^2$$

From this equation the value of x is found to be

$$x = \frac{100\,(\sqrt{n} - 1)}{n - 1}$$

$$\text{Let } n = 9, \text{ then } x = \frac{100\,(\sqrt{9} - 1)}{9 - 1}$$

$$= \frac{100\,(3 - 1)}{9 - 1}$$

$$= \frac{200}{8}$$

$$= 25 \text{ inches.}$$

That therefore is the place where the mark on the bar is to be placed to represent 9 candles. As this mark is 25 in. from the candle, it is, of course, 75 in. from the gas-light. But we know that the square of the greater distance divided by the square of the lesser gives the number of candles, wherefore $\frac{75^2}{25^2}$ must give the quotient 9 if the above be correct: $75^2 = 5625$ and $25^2 = 625$, and by performing the division we obtain the quotient 9.

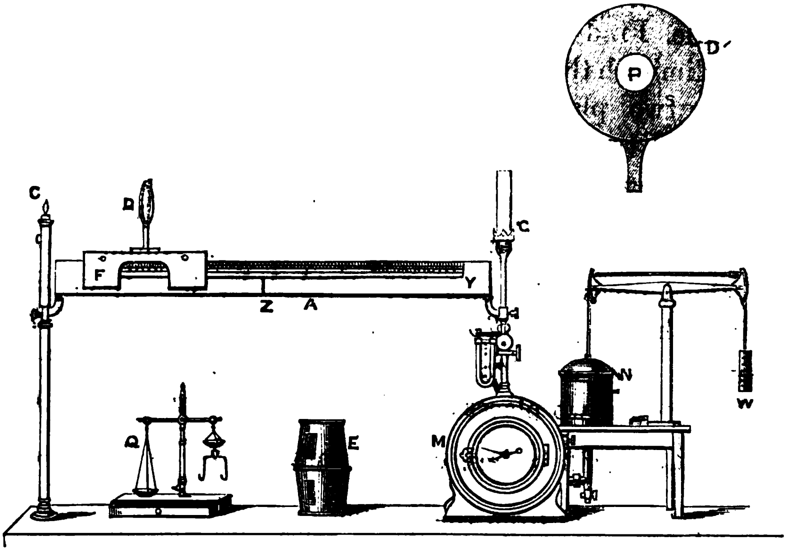
From the above we can obtain a rule for finding the distance of any required number of candles.

Rule. Find the square root of the number of candles, and diminish that square root by 1. Remove the decimal point two places to the right, if there be decimals, and add two cyphers if there be only integer figures, and divide by the number of candles save 1. Taking the above case: The number is 9, of which the square root is 3. This diminished by 1 leaves 2. Adding two cyphers gives 200, and 200 divided by 9 — 1, or 8, gives the quotient 25 inches. To find the place of 7 candles. $\sqrt{7} = 2{\cdot}6458$. Less $1 = 1{\cdot}6458$. Remove decimal point two places $= 164{\cdot}58$. Divide by $7 - 1 = 6$ $\frac{164{\cdot}58}{6} = 27{\cdot}43$ inches, or the mark for 7 candles must be 27·43 inches from the candle.

The following table gives several points thus found:

Number of Candles.	Distance of division mark from Candle.
2	41·42 inches
3	36·61 ,,
4	33·33 ,,
5	30·90 ,,
6	28·99 ,,
7	27·43 ,,
8	26·12 ,,
9	25·00 ,,
16	20·00 ,,
25	16·67 ,,
36	14·28 ,,

Bunsen's photometer and apparatus used therewith:



A is the graduated bar, of which the graduations begin at the centre, Z, and extend right and left from zero, at Z, to 36 candles, at Y.

D, paper screen on its carriage, F, by which it is moved.

D^1, a front view of the screen, where S is the part soaked with spermaceti; and P is the part left untouched.

G is the gas-flame, and C the candle-flame, exactly 100 in. apart.

N is a governor, which can be adjusted by the weights, W, so as to secure combustion of the gas under the same pressure at all times.

M is a test-meter, by which the quantity of gas burnt in a given time is measured.

Q is a balance, by which the candle is weighed, so as to ascertain how much sperm is burnt in a given time.

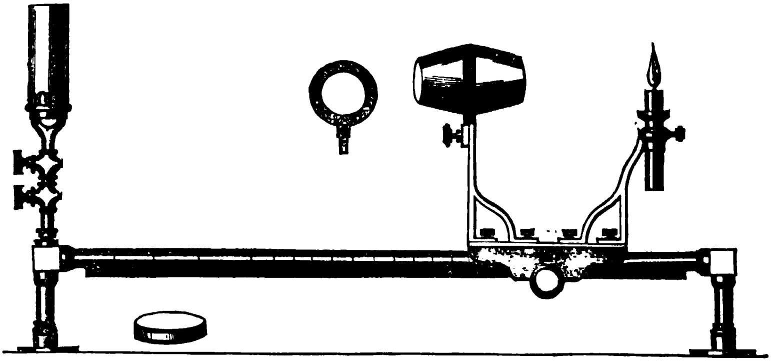
E is a screen, which fits over the disc, D. It has two holes in it, as shown, through which the disc can be seen when the screen is on.

Modifications of the Bunsen Photometer.

MESSRS. CHURCH AND MANN'S.

Messrs. Church and Mann made a photometer in which the disc was placed at a fixed distance of 10 in. from the candle upon a frame which slid backwards and forwards upon the graduated bar. One of the advantages of this arrangement is the shorter bar which is required, viz. one of 5 ft. instead of 8 ft. 4 in. Another advantage is the more easy reading of the disc upon the candle side, owing to its nearness to the candle. On the other hand, this arrangement is open to the well-founded objection of requiring the candle to be moved continually. I have used this photometer a good deal, and, with care, the error from moving the candle is small; but, for general use, those instruments are best which demand from the persons who employ them the minimum of precautions. If the candle were enclosed in a black box, with an opening at the top to allow of the escape of products of combustion, and the frame which carries the candle and disc were moved only by rack-work with a very slow motion, the objections against moving the candle would be almost done away, and the instrument would be satisfactory in use.

This photometer is graduated easily. The division, which indicates any number (n) of candles, is always equal to the square of the distance from the disc to the gas-burner divided by $100 = 10^2$: *i.e.* it is equal to the square of that distance with two figures cut off. Thus the distance of one candle will be 10 in. from the gas-burner, and any division will be found by the formula $\sqrt{100 \cdot n}$, where n represents the number of candles. Thus let it denote 4. Then $\sqrt{100 \times 4} = \sqrt{400} = 20$ in. Let $n = 9$. Then $\sqrt{100 \times 9} = \sqrt{900} = 30$ in.



EVANS'S PHOTOMETER.

Mr. Evans, of the Chartered Gasworks, has constructed a modification of Bunsen's photometer by enclosing the whole in a dark box, and making the disc and burner fixed, and the candle movable. This form is open to the objections which lie against Church and Mann's, and is even more liable to abuse by ignorant people, owing to the candle being enclosed, and their not *seeing* the mischief they do by moving the candle rapidly. I once saw the officers of a corporation testing gas by one of these instruments, and at every fresh observation the candle was whisked a foot or two at a very rapid rate, so that, as they said, they might be sure that one observation should check another. I dare say that the author of this instrument would protest against such ignorant abuse of it, and I mention it only to show how it *may* be and is abused. The instrument is shown on p. 58, and consists of a long wooden box, lined with velvet, having a chimney at each end, under which the gas and candle burn within the box. At the centre of the box, and 50 in. from the gas, the disc is fixed, and this is observed through a hole in the middle of the side of the box. A brass socket carries a candle and moves backwards and forwards in a slot made in the bottom of the box by means of an endless band, to which it is fixed. The band works over two pulleys, and a handle in front of the box enables the observer to move the candle either way without taking his eye off the disc. The scale is engraved on the front of the box, and a pointer from the candle-holder moves over this scale, to indicate the illuminating power observed. In this case the distance from the disc to the gas-burner is the constant quantity 50 in., but the distance from the candle to the disc is variable. The graduation is made thus: Let x equal the distance required for a candle, and n the number of candles. Then

$$n : 1 : : 50^2 : x^2$$

$$\therefore \quad n\,x^2 = 50^2$$

$$\therefore \quad x^2 = \frac{50^2}{n}$$

Let $n = 4$. Then $x^2 = \frac{50^2}{4}$

$$\therefore\ x = \frac{50}{2}$$

$$= 25 \text{ in.}$$

Let $n = 9$. Then $x^2 = \frac{50^2}{9}$

$$\therefore\ x = \frac{50}{3}$$

$$= 16{\cdot}6 \text{ in.}$$

Let $n = 16$. Then $x^2 = \frac{50^2}{16}$

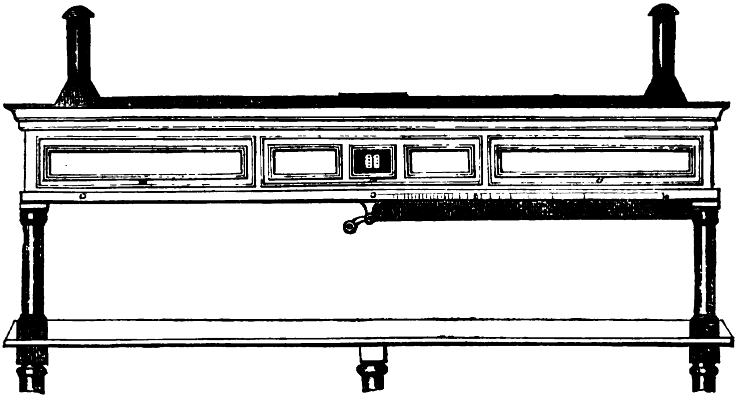
$$\therefore x = \frac{50}{4}$$

$$= 12{\cdot}5 \text{ in.}$$

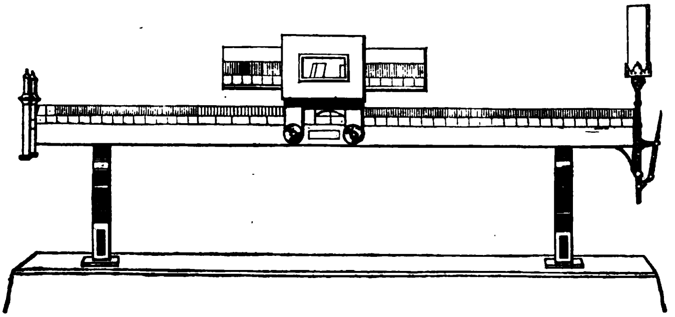
Let $n = 25$. Then $x^2 = \frac{50^2}{25}$

$$\therefore x = \frac{50}{5}$$

$$= 10 \text{ in., \&c.}$$



Another modification has been made by Mr. Sugg, shown by the following engraving:



DR. FYFE'S DURABILITY TEST.

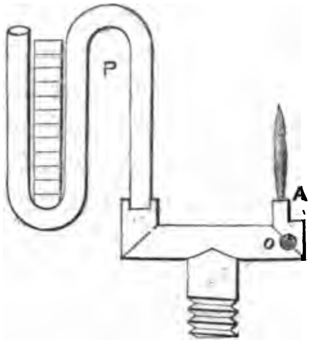
There is a method of examining gas to ascertain its photogenic power, which is commonly known as the durability test of Dr. Fyfe. I first saw it at the Greenock Gasworks, where it is constantly used in preference to any other method for determining the value of the gas there made. The very intelligent engineer of those works assisted Dr. Fyfe in most of his experiments on gas, travelling with him for that purpose during a considerable period. This gentleman had, of course, seen all the methods of valuing gas, but, above all, he preferred and used the durability test. The apparatus employed consists of a clock, a test-meter, a pressure-gauge, a platinum jet-burner, having an aperture $\frac{1}{33}$rd part of an inch (query $\frac{1}{40}$th of an inch) in diameter, and a graduated glass chimney graduated into inches and tenths of an inch, in which the jet burns. I think the chimney is a 7-in. chimney of the ordinary diameter. The gas is burnt from the jet with a flame 4 in. high. Dr. Fyfe's method of applying this test was to combine it with the condensation in volume which gas suffered when brought into contact with chlorine. The gas was passed into a graduated tube over water, and covered so that light could not reach it, when chlorine was passed into the tube with the gas. In the dark, the chlorine combined with the light-giving constituents of the gas, but not with its other constituents; and after a sufficient time for this had elapsed, the excess of chlorine and the condensed matter were removed by water and potash, and the amount of condensation which the gas had suffered was read off by the graduations of the tube. The greater the condensation the better the gas. Dr. Fyfe called this the *quality* test, but he did not rely upon this alone. He combined it with the other, which he called the *durability* test, and said: "Though some have insisted only on the one, and others on the other, yet, unless both be taken into account, we do not arrive at the true value of the gases, and, consequently, cannot compare one with another for the purpose of illumination." He combined the two tests by first ascertaining the amount of condensation by chlorine, then the time required to burn a cubic foot of gas with a 4-in. flame through his jet—*i.e.* the durability, and, multiplying these together, he took the result to be the true value of the gas. The standard of comparison was common coal gas, made from Newcastle coal, which gave a condensation of 4·33 per cent. with chlorine, and required 50·5 minutes to burn a cubic foot. These multiplied together make 218·7, which he called the value of the gas. If this be called unity, as a standard, other gases can be compared with it. Thus, let a gas have a condensation of 7·55 by chlorine, and a durability of 57 minutes, it will have a value of 430·3. Reducing this to the standard, we obtain 1·97 as the standard value of the gas. The following table has been constructed on this principle:

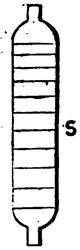
Gas from	Condensation by chlorine.	Durability of a cubic foot.	Relative value.
English caking coal (Newcastle) .	4·33	50′ 30″	1·00
Peareth and Pelton	6·50	50′ 40″	1·50
Yorkshire Parrot	7·66	52′ 30″	1·85
English Cannel (Wigan) . . .	7·55	57′ 0″	1·97
Ramsay Parrot	12·00	62′ 0″	3·40
Midlothian	13·00	60′ 0″	3·56
Lesmahago	17·10	65′ 0″	5·07
Scottish Parrot	15·00	80′ 0″	5·46
Wemyss	19·50	75′ 0″	6·69
Kirkness	20·70	80′ 0″	7·75
Boghead	22·40	81′ 3″	8·32

The values here given accord fairly with photometric valuation in some cases, as, for instance, in the case of Wigan cannel, but they are quite worthless in the case of the Scotch coals. I do not mean that the gases from those coals do not contain illuminating matter capable of giving the light indicated in the table, *if we knew how to consume them so as to convert all their illuminating matter into the maximum of light which it is capable of yielding*, but I mean that it is worthless as an indicator of the *light we now get from those gases.* The figures of the table are supported in a remarkable manner by the yield of oils from the various coals when they are distilled at a low temperature, and, therefore, they have a value as pointing us to a matter of which we are ignorant—viz. the proper manner of burning very rich gas so as to obtain from it all the light it is capable of yielding. Within a few years we have learnt to get 55 per cent. more light from common gas than we could obtain from it previously, and perhaps the progress of knowledge may enable us to realise in practice what *seems* to be the true value of rich cannel gases. At present, no valuation is trustworthy which does not represent the effective light-giving power of the gas as burnt through such burners as can be used with facility by ordinary consumers.

JET PHOTOMETER.

The quantity of gas which will pass through a small aperture at a constant pressure varies with the density of the gas, and, in the case of different gases, the quantity which will pass is inversely as their densities. Attempts have been made to construct a photometer upon this principle, which, by mere inspection of the flame burning before an index, shall show the illuminating power of the gas without calculation, or more than a few moments' expenditure of time. The earliest instrument of this kind which I have seen is the one shown. It was invented by Mr. Alcock, engineer of the Leeds New Gas Company, and in 1836 was presented by Mr. Embleton, mining engineer, of Methley, near Wakefield, to the late Mr. Hedley, of Dublin. In his letter which accompanied the instrument, Mr. Embleton said: "It is Mr. Alcock's invention for testing the quality of gases. The principle of the invention is, that with a certain pressure and with a certain sized aperture in the burner, the length of the flame will determine the quality of the gas. The Leeds gases under an inch pressure give a flame 1½ in. long." I give this quotation, to show that thirty years ago, at least, a photometer on this principle was constructed and used. The instrument is made of brass. A is the jet from which the gas is burnt under a pressure regulated by a governor, and shown by the glass pressure gauge P, which has an engraved scale fixed to it in the usual manner. S is an engraved brass scale which fixes into a small ring, *o*, beside the jet, and is used to read off the length of flame produced by the quantity of gas which passes under the pressure fixed upon for use.



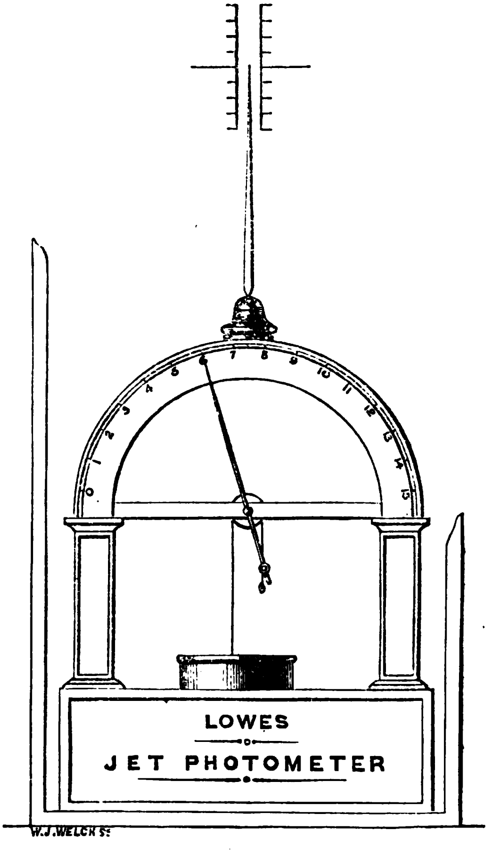


I suspect there is a mistake in the statement that the

Leeds gases gave a flame but 1·5 in. long, under an inch pressure with this instrument. For the last dozen years Leeds has been supplied with good gas, and in a Parliamentary return quoted by Mr. Hughes in 1853, the gas is given as of ·530 specific gravity. Under an inch pressure with Wakefield gas of specific gravity of less than ·430 and an illuminating power of 13 candles, it gives a flame nearly five inches long, wherefore the aperture of the jet must have altered by use, or there must be an error in the statement. Possibly the former, as the box which contained the instrument contained also a common broach, which would soon enlarge the aperture if much used.

In 1860 or 1861, Mr. George Lowe showed me the application of this principle in his study, which he had then recently adopted, and which he used to keep a check upon the quality of gas made at the station of the Chartered Gas Company, which supplied Finsbury. I could not help admiring this fresh instance of the sagacity and zeal with which Mr. Lowe had served the Chartered Company for so many years. By the judicious use of a jet of gas and a scale, he was enabled to ascertain any serious alteration in the quality of the gas during the whole day, from rising to bedtime, instead of being confined in his observation to the hours when he was present at the works. This instrument does not inform the engineer what is the illuminating power of his gas, but it indicates any change in the gas which may require attention, though that change may not involve an alteration in illuminating power. So long as the quality of the gas is unaltered the jet of flame remains the same, but whenever an alteration of *quality* takes place, the jet alters likewise. It is an indicator of *constancy of quality*, not a photometer or light-measure.

This is patent to every chemist without the trouble of an experiment. An increase of carbonic oxide, or of carbonic acid, or of air—all heavy gases—or of hydrogen, a light gas, would alter the *quantity of gas* which would pass through a small aperture at a given pressure, and, therefore, would alter the length of flame, and yet the presence of one or all of these is quite compatible with maintenance of the standard of illuminating power. I have seen this tested by passing the gases of two London Companies through the same jet at the same pressure. These gases gave flames which differed from each other ·75 in., and yet they did not vary in illuminating power by the photometer a quarter of a candle. These gases gave the consumer the same light for a consumption of the same quantity, and yet, having a different chemical constitution, they gave flames of different lengths with the jet. This experiment shows the worthlessness of the jet as a measure of the illuminating power of different gases, as a well-known physical law would show without an experiment. On the other hand, Mr. Lowe's experience shows us its true use—viz. with a minimum of trouble to compare the gas of a company with itself during the absence of the engineer as well as during his presence. Were I a gas engineer, or engaged officially in testing the gas of a company, I should keep a jet constantly burning, and should arrange a self-registering apparatus which would indicate variations of the flame just as pressure is indicated by the curve traced by the pencil on the pressure sheets P. The apparatus now made and sold for this purpose is shown at p. 65. It consists of a governor to regulate the pressure, and a jet with graduated scales to read off the length of the flame. The whole is enclosed in a glass case to protect the flame from currents of air. Any one unwilling to incur expense could make such a thing himself.



PRECAUTIONS IN PHOTOMETRY.

Accuracy in photometry demands the observance of many precautions which are familiar to experienced persons, but which are almost certain to be overlooked by others. In the first place, the quantity of gas burnt must be 5 ft. an hour, or but a few tenths of a foot over or under that quantity. The illuminating value of a foot of gas is not a constant, which will be read upon the photometer however the gas may be burnt, but it is a variable, depending very greatly for its value upon the mode of burning. The burner and chimney for testing are prescribed with reference to a consumption of 5 ft. of gas an hour. But, if 3, 4, 6 ft. an hour be burnt, the value of a foot of gas in standard candles is different in each case. When the quantity of gas is less than 5 ft. an hour the value is below the truth, and so unjust to the company; when above 5 ft. it is in excess of the truth, and so unjust to the consumer. Mr. Banister has given a table which shows this on *one side*, but like all *advocates* who seek to support a case rather than to advance truth, he neglected to show it on the *other side;* in excess as well as in defect; for the consumer as well as for the producer:

Quantity consumed.	Illuminating power as indicated by the photometer.	Reduced to 5 ft. per hour.	Value of 1 ft. of Gas in standard candles.
5·0 feet.	14·0 candles	14·0 candles	2·800
4·0 „	9·5 „	11·9 „	2·375
3·0 „	4·3 „	7·2 „	1·433
2·5 „	2·8 „	5·5 „	1·120
2·0 „	1·0 „	2·5 „	0·500

"The burner employed to obtain these results was a 15-hole Argand burner, with a 7-in. chimney."

Test-meters are provided with an index, which shows the hourly rate of consumption by the observation of a minute, and these indexes are very useful guides to adjust the consumption to *about* the correct quantity; but they are quite worthless beyond this. I have made many careful comparisons with several meters, between the quantity shown by this index and that shown to have been actually burnt upon an hour's consumption, and have always found the discrepancy considerable enough to render the determination of illuminating power inaccurate. I mention this, because I have repeatedly seen the light of gas tested when the quantity burnt was taken only from the minute's indication. It is always well, in delicate observations, to take the quantity of gas from a complete revolution of the pointer over the large index, which usually requires the passage of 6 ft. of gas. There are often defects in the graduations, which render the indications inaccurate, if *taken at parts* of a revolution, and yet the meter may indicate correctly when the pointer has passed through an entire revolution from zero to zero. If an observer have not *himself* verified his meter throughout its course, he will do well to take the time during which the pointer makes a complete revolution of the index, and to calculate the hourly consumption from that. Even with this precaution it may be impossible to secure accuracy, owing to defects in the meter. Few, if any, test-meters are quite correct, and some are seriously defective. It is therefore best to ascertain the error of a meter by very careful testing with a good graduated gas-holder, and to *allow for its error* when calculating the results. The fact of a test-meter being stamped is no proof whatever of its accuracy. Meter-testers cannot afford the time to test a meter of this kind accurately, and by far the best way for a photometrist to be certain of his instruments is to test them himself. I had a good illustration of this in my own experience. A person well known to gas engineers was testing gases in my presence by burning gas against gas through a meter at each end of the photometer. He obtained results which I knew to be erroneous, and I at once challenged the accuracy of his meters. He was indignant at the suggestion, declared that they were made by one of the best makers in London, had been used by Mr. —— as they were then being used, and that to question their accuracy only proved my own incapacity. I nevertheless insisted upon testing the meters (makers, users, and imputations of ignorance notwithstanding) by passing gas through both of them, when it was found that they differed 25 per cent.! It was not then difficult to ascertain on which side lay the ignorance. Some of the persons present on that occasion thought that the discrepancy was known before the meters were used, but this implies such a scandalous attempt at fraud that I could not adopt it without proof. At all events, the defective instrument was repaired, and my results were confirmed fully. *Temperature* is another source of error in meters which I believe is often operative, though but little, if at all, suspected. The general assumption is, that when a test meter has once been found correct, it is ready for use whenever it is wanted, even though it may be in a testing place which is nearly at freezing in the winter, and at 75° Fahr. in the summer. An official meter-tester of considerable experience informs me that instruments which register correctly at 60° Fahr. are quite useless at 32° Fahr. On one occasion he received a large quantity of dry meters to be tested, and as the stores were filled at the time, these meters, in the crates as they were sent, remained in an open yard. The season was winter. When he tested them they were so erroneous that he was obliged to reject them all. Suspecting that there must be some latent cause of this, he kept them a day or two before rejecting them finally, and, to his surprise, this time every one was found correct within the limits allowed by the Sale of Gas Act. In the interval between his two examinations the meters had been kept in the testing-room at about 60° Fahr. He states that temperature does not affect wet meters quite so badly, and yet it affects them seriously. If the facts be as represented, they show that we ought to keep test-meters in a pretty even temperature, and that we cannot rely upon them otherwise. I have no personal experience on this matter, as my own testing-room is kept at a fairly even temperature; but I am so impressed with the importance of accuracy, and the injurious effect of low temperature appears so probable, that I should not test gas in an apartment below 50° Fahr., if any practicable means of bringing it to that temperature were available.

Another fact, not generally known, should be borne in mind. Burners, when first lighted, do not pass the same quantity of gas at the same pressure as they do when they have become thoroughly heated. Then, if we would not include the variable period between lighting and attaining the normal temperature, we must allow the gas to burn till the burner has attained its full temperature before we begin to compute the gas burnt in an examination. Ten or fifteen minutes is sufficient for this purpose. It is also necessary to burn out all gas and air from fittings and meters before we commence an experiment, otherwise we do an injustice to the company whose gas is examined. A comparatively small quantity of air spoils even good gas, and gas itself, when kept, deteriorates rapidly, and it would be most unfair to injure gas by negligence, and then to charge a company with supplying an inferior article. More time than is generally supposed is required to do this thoroughly, especially with dry meters; but it can be done, and ought to be done, before any steps are taken to register the illuminating power.

KIND OF MEASURE TO BE EMPLOYED TO MEASURE THE GAS.

Nothing can compare for accuracy with a really well-made graduated gasholder, and wherever this is attainable it should be used. The cost of a really good gasholder of this description is so great as to put it beyond the reach of most people, and therefore, practically, meters are the instruments used to measure gas when it is tested. These are of two kinds, wet and dry. Wet meters are open to many objections. Their accuracy depends upon their containing exactly the proper quantity of water, any excess or defect rendering them inaccurate. They must be placed and be kept perfectly level, or they will be rendered inaccurate, although well made and properly filled with water. The water they contain acts chemically upon the gas by dissolving part of its illuminating matter on the one hand, and diffusing aqueous vapour into it on the other hand, thus lessening its illuminating power. Again, ammonia, carbonic acid, sulphuretted hydrogen, &c., absorbed from badly purified gas at one time of use, will diffuse into and alter the gas at the next examination, when it may be well purified. Against these are to be set the admirable steadiness of the lights (when a really well-made meter is used), which exceeds anything I have seen with dry meters, and the large and easily read dial—no inconsiderable advantage in a dark room. A dry meter, on the other hand, demands no special precautions of fixing, &c. If it be accurate, its accuracy is totally independent of the person using it.

It contains nothing to act upon the gas, which it transmits to the burner in the same condition as it is received from the gas company. Against this must be set the longer time required to be sure that the meter is quite free from air, and the more serious defect of variability of light when gas is burnt from a dry meter. I admit that the variation is not *great* with the best meters (and, in practice, for house-lighting is not worth notice —in fact, I have used Glover's dry meters for a long time in my house, &c.), but still every trained eye can detect it upon the disc of a photometer, and it is sufficient to affect very seriously the average of a series of observations, if they be taken, first, all at the maximum of each variation, and, secondly, at the minimum. I am aware that an attempt to overcome this has been made by having three measuring chambers instead of two; but even then the steadiness is less than with a wet meter; so that, on the whole, I prefer a wet test-meter, though I prefer a dry meter for general use. It would be unjust to leave the reader to suppose that accuracy is unattainable with a dry meter, for I know the contrary, having used one against competent examiners of gas, when we were entirely ignorant of each other's examination, and having obtained sensibly the same result as they have with other instruments. People may *acquire* the power of using either well, but the wet meter will be found to require less care, on account of its superior steadiness. The smallness of the dials of dry meters is an objection against them, and, when the eye is tired with a long series of observations, matters of this kind become of more importance than they appear to those who have not worked much at the measurement of artificial light.

DESCRIPTION OF A PHOTOMETRIC OBSERVATION.

Those who are unacquainted with photometry, will understand it better from a description of an observation than from any number of rules, and I therefore give a description. But if a person wish to fit himself to estimate the light of gas, I advise him to ask an experienced observer to make a few observations in his presence, and to explain to him fully what is done.

Photometric observations are made in a perfectly dark room, which has no aperture through which any light can penetrate. All the light allowable is that employed in observing. The walls and ceiling of this room are coloured a dead black, or hung with black cloth, to prevent reflexion of light, and to secure the observer against estimating anything but the direct light which falls upon the screen from the gas and candle. The temperature of the room should be kept as near 60° Fahr. as possible. This may be secured by carrying a hot-water pipe through it, or by burning gas continually. Burning the latter through the testing apparatus secures it against air and deteriorated gas, which it always contains if left out of use from one observation to another. The test-meter, photometer, and candle, are all fixed firmly to a bench, which is made so as not to vibrate or shake with the necessary movements and manipulations.

CANDLE.—This is weighed and lighted, and the time and weight are noted. The candle must burn for at least twenty minutes, and sometimes for half an hour, before it is fit for use.

GAS.—The governor is adjusted, if it be not already in a state to secure the burning of the gas under the proper pressure, and the gas is lighted; the small dial is then examined to see how much gas is passing per hour by the quantity consumed per minute. If this small dial show that about 5·1 feet per hour are passing, the gas need not be interfered with, but if it differ anything from this, the taps must be adjusted until this indication is obtained. The time and the state of the large index of the meter are then noted, and as soon as twenty or thirty minutes from the lighting of the candle have elapsed, the observation may be proceeded with. The screen is moved upon the graduated bar of the photometer until both its sides are equally illuminated, and the position of the screen is then read from the graduation on the bar. A small hand looking-glass is used to reflect the light upon the figures while reading them. This glass must be laid *face downwards* when not in use. This first observation is noted, and two minutes afterwards another is taken, and so on for ten observations. These are written under each other in a column, added together, and divided by ten, and the number obtained is the *observed* illuminating power of the gas. If the gas burnt have been exactly 5 ft. per hour, and the consumption of the candle have been exactly 120 grains per hour, or two grains per minute, then the observed illuminating power is the true one. But this hardly ever occurs. We therefore turn off the gas and note the time of doing so: we take the quantity burnt during the observation from the index of the meter and note that, and reduce it to the quantity burnt per hour. If it exceed 5 ft. per hour, the light given by the gas has been too much; and if it be less than 5 ft. per hour, the light given by the gas has been too little, in both of which cases correction is needed, and the observed illuminating power is not the true one, but only an element in the computation of the true one. The candle is extinguished and the time noted. It is then weighed, and the quantity computed to the quantity burnt per hour. If this have been 120 grains, the light of the candle is correct; but if it have been less than 120 grains, the light of the candle has been too little, and therefore the gas measured by it has appeared too good; whereas, on the other hand, if the quantity burnt has exceeded 120 grains, the light of the candle has been too great, and the gas measured by it has appeared too bad. In both these cases correction is needed, and the observed illuminating power is not the true one.

If the candle have burnt 123 grains instead of 120, and the observed illuminating power were fourteen candles, the correction for the error of the candle is made thus:

$$\begin{array}{ccccccc} \text{Normal quantity.} & & \text{Quantity burnt.} & & \text{Observed illuminating power.} & & \text{True illuminating power.} \\ 120 & : & 123 & :: & 14 & : & x \end{array}$$

$$\therefore\ 120\,x = 123 \times 14$$

$$= 1722$$

$$\therefore\ x = \frac{1722}{120}$$

$$= 14{\cdot}35$$

The observed illuminating power reduced to standard candles.

Rule to correct for error of candle.

Multiply the observed illuminating power by the grains of candle consumed per hour, and divide by 120, to find what the observed illuminating power would have been if the candle had burnt the standard quantity of 120 grains per hour.

If the gas burnt in the experiment differed from 5 ft. an hour, a correction is needed for that. Suppose that the quantity burnt were 5·5 ft., then the correction is made thus:

Normal quantity.		Quantity burnt.		True illuminating power.		Observed illuminating power corrected for error of candle.
5	:	5·5	::	x	:	14·35

$$\therefore \quad 5{\cdot}5\,x = 14{\cdot}35 \times 5$$

$$= 71{\cdot}75$$

$$\therefore \quad x = \frac{71{\cdot}75}{5{\cdot}5}$$

$$13{\cdot}04$$

The true illuminating power of the gas in standard candles.

Rule to correct for error of gas.

Multiply the observed illuminating power corrected for error of candle by 5, and divide by the quantity of gas burnt per hour. This gives what would have been obtained by experiment, if the gas had been burnt at the rate of 5 ft. an hour, and the candle had burnt exactly 120 grains an hour.

Tables have been published to show at a glance, without any calculation, the true illuminating value of the gas when the consumption of the gas and candle were abnormal within certain limits. They are useful as saving time where many observations are made, and also as checks upon calculation.

CHAPTER IV.

PLACE OF TESTING GAS.

The first thing an examiner of gas would wish settled, as soon as he was appointed, would be the *place* at which his examination should be made. Common sense would indicate, that as the testing was designed for the protection of the consumer, and for his protection only, the testing might be made at the house of any person who used gas. When the law prescribes a certain minimum of illuminating power, below which the gas of a given company shall not fall, it must be taken to intend that this minimum shall apply to all the gas supplied to all the consumers, at any time, and at any place, within the limits of its main. This is the only rational meaning of the provision, and nothing but express words in the statute regulating the standard of the gas could be taken to override it. It is absurd to suppose that Parliament meant to take care of consumers in one part of a company's district, and to neglect those in another part, and therefore the testing should take place in such a portion of the district as will secure gas of the prescribed quality to *all customers*. The Metropolis Gas Act of 1860 fixes this place at a thousand yards from the works whence the gas is supplied. This distance was taken as a compromise between the statements that gas at the works is better than it is after it has travelled through a great length of main, and therefore testing at the works was too favourable to the gas company, whereas testing at the extremity of the district, where the gas was said to have undergone its maximum of deterioration, was too favourable to the consumer. Having gas of the proper illuminating power at the extremity of a district, it was said, would give too much for the money, while having it of the proper illuminating power at the works only, would give too little.

Assuming as fact all that was said about deterioration of gas—which is by no means admitted—it must be clear to thoughtful people that the consumers who lived towards the verge of a district had as much right to be supplied with gas of the proper quality as those who lived near the gasworks. A man's shilling's-worth surely cannot be regarded as a variable quantity depending upon the locality of his residence! A grocer or butcher would have the same right to accommodate the standard pound to the distance of his customers from the shop; and while he gave the honest sixteen ounces to those in the same street, he would gradually pass through fifteen, fourteen, thirteen, twelve ounces, according as the customer had the misfortune to be distant from the source of supply. If this would be wrong—and who doubts it—in the case of meat and tea, what is it in the case of gas? Clearly the examiner of gas ought to be at liberty to examine gas at any place in which it is sold by the company whom he is appointed to check, and the fixed standard should be held to apply in every part of the district and to every foot of gas sold.

The question whether a gas company is to deliver gas of standard quality to its consumers, or is merely to make gas of the standard quality before it leaves the works, is really whether *the company or the consumer is to incur the risk of distribution.* It is said that gas becomes deteriorated by passing through a long length of main, and therefore that the company should not be liable to have the gas tested after it had passed the mains, but before it enters them; or, at all events, before it has passed far from the works. This question can be settled by analogous cases. Milk is a fluid very liable to be spilt in transit from the dairy to the consumer. If some be spilt, is the consumer to take short measure, or to have the loss supplied by water? The pails and the people who carry them belong to the dairyman, and it would startle us to be told that the consumer was the proper person to suffer for their negligence or misfortune. Similarly, wine-bottles are frangible, and wine from broken bottles is lost. A customer would be slow to accept ten bottles for a dozen, if the wine-merchant's porter broke two bottles, and he would be equally unwilling to consider two bottles of water an equivalent for the lost wine. Why should not the customer equally decline to accept deteriorated gas, if it do deteriorate as alleged, when the gas mains are the property of the gas company, and are under the exclusive control of their officers? Rather, why is one law to apply to *quantity* and another law to *quality?* Gas companies lose enormous quantities of gas by leakage from their mains, but where they lose as much as 20 per cent. thus, they never propose to give 80 and charge for 100 ft. The meter is placed on the consumer's premises, and the leakage has taken place before the gas gets there to be measured. Why should not the *quality* be determinable at the same place as the quantity? Is it reasonable that they who own and control the distributory apparatus, should throw upon others any of the consequences of the distribution? Obviously not, and reason requires that the standard illuminating power fixed by Parliament shall be understood as referring to gas at the premises of the consumer. This point was contested obstinately in the case of the Metropolis Gas Act, and it is contested by the advocates of gas companies, whenever the point is mooted before a parliamentary committee. Why? It cannot be on the ground of the deterioration suffered by the gas in travelling, because the contention was that that deterioration had taken place in the first thousand yards of passage through the mains, and that the light given at a testing-station at that distance from the works, fairly represented the value of the gas throughout a whole district. Why, then, is the point contended for? Examination of the Act shows, not only that the testing station is to be at this short distance from the works, but that if, on testing, the gas be found below the standard, *three hours' notice* are to be given to the company before a testing can be made, upon which to found a successful application for penalties. These two things are connected in people's minds thus. It is said upon oath before parliamentary committees, that if gas should happen to be below the standard, and notice be given to the officers of a company that the gas will be tested in three hours, they have it in their power to alter the gas, so that at the end of the time named it shall be of proper illuminating power, and therefore no conviction could follow, though they might be selling an inferior article at all other times. No testing will sustain a case for penalties except one made after the three hours' notice. It is of vital importance to know whether the officers of a gas company under notice really could alter the quality of the gas within the time specified. There is overwhelming evidence that they *could* do so — I do not assert that they *would*—and therefore that any such provision in an Act of Parliament as a testing-station a thousand yards from the works with three hours' notice, is worse than idle: it is a legislative shield devised for the purpose of rendering the penalty imposed impossible of recovery.

On the other hand, it is pertinaciously denied that any company could alter its gas within the time of notice, and elaborate calculations have been produced before parliamentary committees to *prove* that they cannot. The arguments derived from figures are proverbially capable of proving anything. Grant a man his assumptions, and he must be most unskilful if he cannot prove his point beyond all answer. But the question in such a case as this is wholly one of experiment. Arguments derived from the flow of gas in pipes (even if they were worth much in themselves, which they are not) are entirely beside the question, and nothing is trustworthy but a carefully-made experiment, supervised, if need be, by sceptics and enemies, as well as by prejudiced friends. If a man say, I can alter the gas of a given company, or of any company, within a given time, and another denies his statement, the matter has passed beyond the reach of argument. His statement is true or false: experiment, and it alone, can decide whether. He who loves truth will be ready at once to submit the case to the unerring test of a carefully-made and well-watched experiment.

The question was decided years before it was started as a controverted one between gas companies and their opponents when struggling in Parliament; and two cases which have actually happened will settle this point. One is that of a large manufacturing town, having a flourishing gas company, with well-managed works. Their engineer is a man of great common sense, anxious to deal fairly by, and satisfy, his customers. He is respectable and respected. Some years ago, his plant was beneath the demand occasionally made for gas, and at times he was sorely puzzled how to supply gas enough for such an emergency as a sudden fog. Troops of messengers used to be sent to the works on such occasions, to demand more gas; but more gas he could not give them, for he had it not to give. Ordinary demand taxed his plant to the utmost, and meet an extraordinary demand by an increased supply he could not. He resolved, therefore, to give in *quality* what he could not give in *quantity;* and to this end kept a large supply of the best naphtha. Directly an emergency came, his naphtha was poured into his mains within the works, and in a quarter of an hour complaints were at an end. The consumers got the light they wanted, and were under the impression that the additional light came from additional gas. This is no affair of naphthalising a few feet of gas in an experiment; but in a work as large as some of the London works, and at a time of extreme pressure, the illuminating power of the gas was raised so much as to relieve a town's complaint of deficient light, and the whole was accomplished in a few minutes; in fact, before the messengers arrived at home from the gasworks.

This case is not produced by way of censure, as if there were anything blameworthy in what was done; on the contrary, the engineer deserves the highest praise for his liberality and thoughtful good sense. His customers wanted light at a pinch, and he could not give it them through gas. If they had not got that light, their works must have stood still, and this would have issued in a new company. He therefore spent a few pounds to meet the demand, and, to this day, enjoys a monopoly, which no one envies. This case is produced to prove, and it proves conclusively, that a testing place near a gaswork, *with any notice whatever*, is an indemnity to the gas company in whose Act it is prescribed. It proves that the officers of a gas company may sell gas of their own quality, and yet defeat an examiner in his testing for penalties whenever he gives them notice. If testing is to be worth anything at all, it must be made without notice, and anywhere in the district.

The next case is one wherein no naphtha was used, but where a rise of illuminating power was obtained by suddenly drawing and charging a large number of retorts. The gas of a particular company was being tested by myself and several other persons, and was found to have an illuminating power of 9·83 candles. The parliamentary standard was 12 candles. This was felt to be disgraceful, and a person connected with the company left the testing-room, and told the manager privately the fact. Another experiment was then made, which lasted half an hour; and during that time the illuminating power increased to 12·8 candles! Five persons who were present at the time were astonished, and all agreed that something had been done to the gas. Rigid inquiry was made, and it turned out that twenty retorts had been drawn and re-charged as rapidly as possible (it was summer-time, and there were but forty retorts in use), and the good gas produced at the early stage of the distillation was that which had been sent to the photometer. The whole of this happened within half an hour. Orders were given to have the gas then being made sent into another gasholder; and upon testing the gas again, it was found of the same illuminating power as before the retorts were re-charged.

This deceit was accomplished by using the ordinary common coal from which the poor gas was made. What might have been accomplished if half a ton of Boghead coal had been used, or if rosin or fat had been thrown into some of the retorts?

Two dissimilar cases, occurring at considerable intervals of time and space, one thoroughly honest and praiseworthy, the other dishonest and blameworthy, join to prove that gas testing is a mockery, if the place of testing be near the works, and notice be given of an intention to test.

I believe that the companies supplying the City of London have waived their rights in this matter, and their having done so is a credit to them. It is a proof of *bona fides*. But at any time they may insist upon their right, as may any company who gets such a clause inserted in its Act. No such clauses, therefore, should be permitted, but gas should be tested by a competent officer, at any time, and at any place.

Nor is this any hardship upon gas companies, if the experience of the officers of the largest company in London be reliable. We are told that their gas, several miles from the works, is actually of a higher illuminating power than when it is sent out. The same burner, meter, &c., have been transported, to prevent the possibility of mistake, and yet the increase of illuminating power has been found constant.

I have no doubt of the accuracy of the above statement, though it is very often ridiculed by those who ought to know better than even to doubt it. The gas is purified and sent out at a higher temperature than it is at when tested four miles from the works, and therefore it is less dense, and contains more aqueous vapour than when it is examined after passing through the mains. Besides, in cooling and depositing part of the vapour of water which it contains, it also deposits part of its carbonic acid in solution in the deposited water, as may be shown by an analysis. Not only, therefore, do I believe the statement of the intelligent engineer of the works in question, but I see that his statement must be true, and that the facts cannot be otherwise than as he represents them.

Not long ago, Dr. Letheby furnished evidence that the London gas companies do add naphtha to their gas in the mains, which they cannot do without *pro tanto* improving their gas; and, therefore, what is argued as a possibility is now to be accepted as a *proved fact.* I neither say nor think that they add naphtha fraudulently; but I do say that the only way to put the whole matter upon a proper footing, and beyond suspicion, is for testing, everywhere, to take place without any notice to the officers of a company. The passage I refer to forms part of a lecture delivered at Birmingham, before the Society of Gas Engineers:

"Benzole exists in coal gas in very variable proportions. At times it is but barely discoverable, and at other times it is present in large quantity. It is often present in London gas to a considerable extent, and I attribute it to the practice which is occasionally followed of pouring light naphtha into the mains, for the purpose of removing naphthalin. I here show you a specimen of nitrobenzole obtained from only 15 cubic feet of gas."

The late Mr. Banister was the superintendent of the Westminster station of the Chartered Gas Company, and very naturally viewed all questions relative to gas from the side of the company. A passage in his "Gas Manipulation" explains the anxiety of gas companies to have their gas tested *at the works*, or very near them. The passage I refer to is: "I cannot too strongly insist upon the necessity of operating upon the gas in the gasholder *as soon as possible after it has come over from the retort;* for, if left too long, the gas being in contact with so large a surface of water, a considerable portion of the hydrocarbon vapour will be absorbed, and a great deal of the ammonia present abstracted, in consequence of its rapid absorption by water. After gas has been standing in the gasholder for twelve hours, I have found a diminution in illuminating power of 23 per cent. from this cause."

I am quite aware that this extract refers to gas made for experiments, but most people will think that what relates to such gas cannot be much misapplied when it is applied to gas in general. People will think, too, that the only light-giving matter useful to a gas consumer is that which is delivered to him, and that this only should be considered in estimating the illuminating value of gas. If gas be tested and then deteriorated by standing in the gasholder, the testing is perfectly useless to the consumer as a guide to the light-giving power of what he receives and burns.

The various and contradictory statements relative to the influence of travelling upon the illuminating power of gas have received a curious addition recently. Before a Committee of the House of Lords which sat in 1865 upon the Hanley Gas Consumers Bill, Dr. Letheby gave the following evidence:

2723. Where did you test the gas; was it done at the office of the Company?—In the town.

2724. How far is that from the works?—I think about 2½ miles from one of the works, and about a mile from the other.

2725. One mile from the Etruria Works?—Yes; and 2½ miles from the other.

2728. Did you make five sets of experiments?—Yes, I did; I made three experiments on the 16th, and two on the 17th.

2729. What were the results?—The least illuminating power was 14·59 candles, and the highest illuminating power was 16·42, the average being 15·66.

2730. Did you make a second visit to Hanley?—Yes, I did. I made another visit on the 10th and 11th of February.

2731. What was the result?—I made four examinations at the office (this was in the town), and the lowest was 14·12, the highest being 16·24, and the average 15·1. I also examined the gas in the works at Etruria, and found it was 15·56 candles, *so that there was very little difference* between the gas *at the works and the gas at the office.*

2754. Would it not make some difference whether the gas was tested at the works, or a mile off?—*It depends on the gas;* but, in this instance, I did not make the experiments a mile off.

2755. I ask you as a general question, if you test gas a mile off the works, do you not lose something like one candle?—Yes; *but I know instances where gas, properly* *purified, has not lost anything in illuminating power after travelling six miles.*

These experiments were made for the purpose of being given in evidence, and therefore, of course, with great care. This gives them additional value. They prove beyond question that gas loses less than 3½ per cent. by travelling from the works to the town, and, therefore, that it can be supplied to consumers, situated at a distance, of substantially the same illuminating power as it has when it leaves the works.

But what, then, becomes of the statement so often made to consumers, public authorities, and parliamentary committees, that gas must leave the works two candles above the standard fixed, or it cannot be delivered to the consumers of the standard quality? In other words, that 14-candle gas at the works becomes 12-candle gas at the consumers' premises, having lost the light-giving power of two candles by travelling. And of that other statement, that it ought to be tested within 1000 yards of the works, in fairness to the company? They are all disposed of by the evidence I have quoted. And it is worthy of remark, they are disposed of by a witness called for the gas company whose gas was examined. Dr. Letheby was called by the British Gas Company, who supplied Hanley, and not by the Consumers' Company, which sought power to supply it, and, therefore, we are sure that the evidence was given simply because the truth required it. The Hanley gas loses less than 3½ per cent. by transit, and there are other "*instances where gas, properly purified, has not lost anything in illuminating power after travelling six miles.*" This is the kind of gas consumers want, and to secure it the testings should always take place in the centre of the district of consumption. All gas Acts ought to contain a clause to allow testings to be made anywhere within the district supplied. The companies themselves have proved the fairness of the provision.

A curious illustration of the worthlessness of testings made at gasworks is furnished by an article in the *Chemical News*, of July 13, 1861. The article had no reference to this question, but dealt only with the amount of sulphur in the gas sold by a single London company, and its testimony is, therefore, the more valuable. It seems that the Commercial Gas Company was anxious to have something to reassure their consumers, after the disclosures made in 1860, when the Metropolitan Gas Bill was under consideration. They accordingly engaged Mr. Versmann to examine their gas for sulphur, and published his report. He examined the gas AT THE WORKS, and as the result of six separate examinations, he found a mean of 4·67 grains of sulphur in 100 cubic feet of gas. After this report was published, six other examinations of that company's gas were made in a laboratory, to which it was supplied for use in the regular way, as it was supplied to all other customers. The mean of these examinations gave 57·38 grains of sulphur per 100 cubic feet of gas; a difference which is at least remarkable. The latter testing was made without the knowledge of the company, upon gas as supplied to the public. The former was made at the works, with the privity of the company's officers, upon gas as supplied to the analyst. Can such examinations deserve confidence? Ought they to be allowed? Public judgment will, I think, answer these questions in the negative.

While these sheets were being printed, I saw, for the first time, a published account of the alteration of illuminating power, which took place at Uxbridge on the occasion of testing the gas of the Uxbridge Gas Company, January 19, 1866. The Act of that company requires six hours' previous notice before the gas can be tested for penalties, and the testing must take place at the works. The standard of illuminating power is 12 candles. Notice to test was given at ten o'clock in the morning, so that no testing for penalties could take place before four o'clock in the afternoon. Between two and four o'clock the gas in the town was tested, and was found to have an illuminating power of 12·48 candles; *i.e.* just half a candle above the required standard. Upon testing the gas at the works after the expiration of the notice, it was found to equal 15·78 candles!—*i.e.* 3·78 candles in excess. In fact, the gas was *prepared* to be tested, and the contractor who manufactures the gas for the company made no secret of the fact. He wished to prove how easy it is to alter the illuminating power of gas if notice be given, and, consequently, he ordered some cannel coal to be put into the retorts, to prepare gas of an extra illuminating power to be tested. The thing was done with the most perfect openness, as the examiner was told of it while at the works, and was designed to show how perfectly worthless a test is, if a gas company can demand notice before its gas can be tested for penalties. As this was a case of adverse testing, it is exceedingly valuable.

CHAPTER V.

TEST CANDLES—THE STANDARD FOR MEASURING GAS-LIGHT.

SOLIDS are bought and sold by standard weights. Fluids by standard measures of capacity; and such goods as cloth, calico, &c., by standard measures of length. Gas in like manner is sold by a standard of quantity, viz. a cubic foot, and by a standard of *quality* also. The reason for the introduction of the quality standard is found in the fact that *light* is what we really want in the absence of the sun, and that when we purchase gas we do not in reality want the *gas*, but the light which the gas will give when burnt. Parliament has therefore ordered that gas shall be sold by the double standard of measure and quality, and the standard of quality fixed upon is a sperm candle which burns 120 grains of sperm an hour, or two grains a minute. In the early days of gas-lighting the standard of illuminating power varied a good deal, and we find the older writers comparing gas with candles (dips) so many to the pound, with mould candles so many to the pound, with wax candles, and finally with sperm. This was thought to be a good substance to fix as a standard, when Parliament was seeking one; and, as well-made genuine sperm candles of six to the pound burnt nearly 120 grains per hour, this material and quantity were taken as the measure of the luminosity of gases sold for illumination. If the candle burnt a little more or a little less than the fixed quantity it was allowed for in proportion, and the light actually measured was computed for a consumption of 120 grains an hour. This standard and computation involved several suppositions, some of which are positively false, and others far from satisfactorily established. It obviously assumes that all sperm candles of six to the pound will consume nearly the proper weight of sperm per hour, and *that the illumination for equal consumptions of sperm by different candles will be equal.* It assumes, also, that candles which are sold as sperm will be really such; and the proportional reduction of the quantity consumed to the standard of 120 grains per hour assumes that each grain of sperm has the same illuminating value, whether it is burnt at the rate of 110 grains an hour, as in some candles, at the rate of 120 grains an hour, as in normal candles, or at the rate of 140 grains an hour, as not unfrequently happens to be the rate of consumption of candles six to the pound, employed in photometry.

The most material of these assumptions is *the uniform illuminating value of like quantities of sperm when burnt in different candles.* If this fail, the rest may be disregarded, and the standard is worthless. To determine this, and eliminate sources of error, I have divided photometric sperm candles into three portions, one of which was thinned by scraping, another of which was left untouched, and the third of which was thickened by the sperm scraped from the thinned portion and from other portions of the same candle. In this case the sperm is the same in all the portions, and the wick likewise is the same, which might not be the case were portions taken from *different* candles, so that the quality of the sperm and the thickness and quality of the wick are the same in each portion tested. The only difference is in the *altered relation* between the wick and the sperm in the three portions. Of course when the light was determined, it was calculated from the quantity of sperm actually burnt to the normal quantity of 120 grains per hour.

Section A, from the middle of candle, had a diameter of $\frac{13}{16}$ths = ·8125 in.

Section B, from the top of candle, had a diameter of $\frac{10}{16}$ths = ·625 in.

Section C, from the bottom of candle, had a diameter of 1 in. = 1·000 in.

Section A burnt 70 grains in 35 minutes, which is the proper quantity of 120 grains an hour. A gas flame was made to be exactly eight times the illuminating power of this candle, and this flame was not altered during the experiments.

Section B burnt 57·4 grains in 30 minutes, or 114·8 grains an hour. The gas gave 9·6 times the light of this candle, and when the light is calculated for the standard consumption of 120 grains an hour, it is found that the flame equalled 9·18 times the standard candle. In the former experiment it equalled but 8 times the standard candle.

Section C burnt 61·2 grains in 30 minutes, which is equal to 122·4 grains an hour. The gas flame was 7·8 times the light of this candle, and when calculated for the standard consumption of 120 grains, 7·95 times.

Calling the light given by the gas 100 as valued by candle A, it becomes 114·8 as valued by candle B, and 99·4 as valued by candle C. In a tabular form :—

Section of Candle.	Diameter of Candle.	Consumption per Hour.	Illuminating power of same gas flame.
A	8·125 in.	120 grains	100
B	·625 in.	114·8 grains	114·8
C	1·000 in.	122·4 grains	99·4

Here is a large difference of 15·4 between B and C, without there being any real difference in the light given by the gas flame. I know it will be objected that I altered the candle to produce this result, and that the thinner candle is not the candle intended by the Act of Parliament, and this objection will be made by the very persons who advocate the use of *improved burners* for testing gas. But surely the argument is all with me, and against the objectors. Parliament sanctioned a burner and a candle which were known and commonly used at the time they were adopted for testing. Since that time the burner has been altered by a contraction of the internal aperture, and an enlargement of the holes, and in this *improved* form it makes the gas appear of a higher illuminating power than it would appear if tested by the burner in use when the standard was fixed. It is maintained that this *improved* burner should be used in testing. For argument's sake, I admit this. But science cannot stop at an improved burner. It claims now the use of an *improved candle*, which is a valuable auxiliary to the burner, and makes the gas appear still better than it could possibly appear if tested by the old-fashioned candle intended by Parliament. The candle is still sperm, still six to the pound, only it is a little longer and thinner than the old-fashioned one which bore the above title. *Technically*, the candle corresponds to the legal standard, it is a sperm candle, six to the pound; *morally*, it departs from it widely. Technically, the improved burner corresponds to the legal standard, it is a fifteen-hole Argand, and has a 7-in. chimney; morally, it departs from the standard widely.

To estimate this matter rightly, the reader should remember a provision in most Gas Acts, that *the company* shall supply the testing apparatus. Suppose a chemist to be supplied with an improved burner and with improved candles, I apprehend that he would be bound to use what was supplied to him and to report what he found. In such a case, what difference would there be between the illuminating power of the gas measured by the burner and candle sanctioned by the Legislature, and the illuminating power of the *same* gas measured by the improved burner and candle? The steatite burner, No. 3, p. 113, converts 100 of light into 117 of a common candle. The improved candle converts 100 of a common candle into 114·8, so that what would have appeared as 100 by the tests intended by Parliament appears as 131·8 by the improved tests. Such would be the report of the examiner of gas. But what increase of light is obtained by the consumers? Literally, none. Their fittings have not been altered. They use what they always used, and obtain the same light, whether the examiner report the gas to have the value of 100, as he would have done before 1860, or 131·8 as he might be bound to do now.

Most persons will think that a standard so illusory should be changed without delay, and that it would be better to do away with a standard of quality altogether than leave matters as they are. There is no necessity for this indefiniteness. Standards could be defined with sufficient precision to render them accurate in competent and honest hands.

I select the above experiment because it is the *least* favourable to the thinned candle which I have made, and because the unaltered portion burnt exactly the normal quantity of 120 grains an hour; but I must guard against having it thought that thinning of the candle is always accompanied by a diminished consumption, as it was in this case. Sometimes the reverse happens. On one occasion I thinned part of a candle, and determined the illuminating power of the gas by it as compared with the unthinned portion, when I found that the portion thinned burnt 130 grains an hour—more, in fact, than the unaltered portion burnt—and that the thinned portion gave so much less light, that the gas measured by it gave the light of more than 18 standard candles, whereas by the unaltered part of the *same candle* it gave the light of rather less than 14 candles.

These experiments prove clearly that a standard photometric candle ought to be made, so that it shall burn as nearly 120 grains of sperm as possible, and that it shall burn this so as to give *the greatest quantity of light which can be obtained by its combustion.* There is no difficulty in making candles of this character. The best of cotton must be used for wick, instead of the inferior sort, now frequently employed. The candle must be cylindrical instead of conical; and, in short, the object aimed at in the manufacture must be the production of an article which will be of uniform quality and will answer its purpose. Cost in manufacturing test candles is not worth consideration where so few are used, and such great interests depend upon accuracy.

What has been said will serve to explain a circumstance which gave rise to much comment, both in and out of Parliament, last year. Persons conversant with gas matters are aware that for ten years Dr. Letheby has used his sperm candles in metallic cases. The cases contained a spring to thrust up the candle and keep it in a constant position—a convenience which every photometrist will appreciate. This spring forced the shoulder of the candle against the metal of the case, and thus chilled the sperm, so that it did not melt into the cup around the wick in the same manner as it would have done had the candle burnt in the free air without contact with metal.* I well remember being consulted by several persons relative to this mode of burning the candle, when it was first introduced, and systematically pointing out to them in private what was at last made public very curiously. Dr. Letheby was requested to examine the gas of the Birmingham and Staffordshire Gas Company. He did so, and took down his own apparatus. He found the illuminating power to be one number of candles. Another gentleman went down to examine the gas, and found its illuminating power to be a different number of candles. This led to comparison, and each held confidently to his own results. To ascertain who was right, another gentleman was requested to examine the gas, and, if possible, account for the discrepancy. He did so, and it was found in the different illuminating value of the same candle when burnt with and without the case. A given quantity of sperm burnt in a case gave less light than when burnt without a case, and consequently made the gas appear better than it really was. My thinned candle has explained the reason of this. Every observation made with cased candles was against the consumers, in whose interest the testing was established.

The author of the "Chemistry of Artificial Light," in Orr's Circle of the Sciences (Griffin, London, 1860), has described candles amongst other sources of artificial light, and what is said of some bears upon photometry:

"SPERMACETI CANDLES contain about 3 per cent. of wax, which is added to break the grain, or to prevent crystallisation. A sperm candle of six to the pound burns with great regularity, if it is properly made; *but those in commerce at the present time are but poor examples of what such candles should be*, for they range in combustion between 120 and 146 grains per hour. A spermaceti candle of six to the pound, burning at the rate of 120 grains per hour, is generally taken as the standard of comparison for all other illuminating agents, for the light emitted is clear, white, and very brilliant."

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* The candle, in fact, was thinned by the metal ring against which its shoulder was pressed by the spring.

"WAX CANDLES are rarely composed of pure wax, but consist of wax and stearine or stearic acid in various proportions. The candles of commerce are very irregular in combustion; in fact, they burn from 135 to 175 grains of wax per hour; and they give a *light which varies from one up to three*, according to the condition of the wick. At the best of times the light of wax is, weight for weight consumed, about one-sixth less than that of spermaceti."

"PARAFFIN CANDLES.—The candles which we have burnt are consumed at the rate of from 166 to 185 grains per hour, and the light emitted has, weight for weight of candle consumed, been exactly *one-twelfth less than that of sperm.*"

"STEARIC ACID CANDLES.—The candles of six to the pound burn at the rate of from 140 to 144 grains per hour, and they *give a light as nearly as possible the same as sperm.* If there be any difference, the light of sperm is a little greater, and that of stearic acid a little whiter. When calculated into an average consumption of 120 grains per hour, it will be found that for equal weight consumed, 15 sperm candles will give the light of 16·5 stearic."

The evidence given on the "Birmingham and District Gas Consumers' Bill," before the Lords' Committee (March, 1864), contains admissions which ought to be generally known. I have extracted portions which relate to test-burners and candles.

Mr. G. J. Parkinson, an officer of the Birmingham Gas Company, says: "My observations were made with the old-fashioned metallic Argand burner, having 15 holes, and a 7-in. chimney. From July to December, 1861, the illuminating power was equal on the average to 12·44 sperm candles; from January to December, 1862, it was 12·55; and from January to July, 1863, 12·92. I am aware that another burner is commonly used in London for photometric purposes, called the steatite or Sugg's burner. If I had made my observations with that, they would have shown 10 per cent. additional illuminating power in the gas, making the result of the observations in 1861 equal to 13·68; in 1862, to 13·8; and in 1863, to 14·21 sperm candles."

This will be better seen in a tabular form, and it should be remembered that Mr. Parkinson's supposition of 10 per cent. difference in the results is under the mark; it is really 17 per cent.:

	1861.	1862.	1863.
Old burner gave	12·44	12·55	12·92
New burner would have given, according to the witness . .	13·68	13·80	14·21

Dr. Odling, lecturer on chemistry at St. Thomas's Hospital:

"I have examined the gas of the Birmingham companies on several occasions. I used exactly the same instruments which Mr. Parkinson habitually employs. The mean of all my observations gave an illuminating power of 13·8 sperm candles. Comparing the burner used in these experiments with the steatite or Sugg's burner, I should say that the latter would give, on the average, 10 per cent. higher results; so that the mean would be 15 instead of 13·8 candles."

Old burner gave, mean	13·8
New burner would have given. .	15·0

Dr. Hofmann, professor of chemistry, Royal School of Mines:

"I experimented, on the 12th, with Mr. Parkinson, to ascertain the illuminating power. The result was 14·63 candles. By Dr. Letheby's photometer, with cased candles, the result was an illuminating power of 17·36 candles. The cased candles are candles forced into a case with an opening at the top just large enough for the wick to pass through, and the object is to keep the flame of the candle always at the same elevation. I have made experiments on the difference of light emitted by cased candles and candles not enclosed, and find that the proportion of light is as 185 to 200, the cased candles invariably giving the higher result. I have made experiments on the difference of light obtained in the common Argand burner and the steatite burner, and I have satisfied myself that the latter, which is generally used in photometric observations in London, gives a higher result; but my experiments have not been sufficiently numerous to allow me to state the exact percentage by which the light is increased."

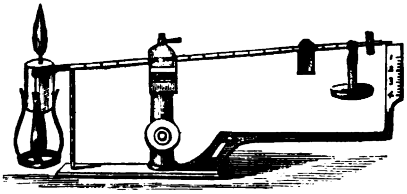
With ordinary candles, the gas equalled 14·63 candles.
With cased candles, the same gas equalled 17·36 candles.

That is, the consumer received exactly the same light for his money whether the gas was reported to be 14·63 or 17·36 candle gas! The only difference was in the mode of estimating the illuminating power.

The idea of keeping the flame of the candle opposite the centre of the disc was a good one, notwithstanding the failure in the instance of casing; and I have now the satisfaction of publishing an apparatus which secures the proper position of the candle without in any way interfering with its free combustion in the air, and which, at the same time, gives the important additional advantage of weighing the candle so as to take account only of sperm burnt normally.

NEW PHOTOMETRIC CANDLE-BALANCE AND ELEVATOR.

Accuracy in photometry cannot be attained unless the flame of the candle employed is opposite the centre of the disc, and the candle is weighed while burning without being interfered with in any way. The first weighing must not take place until the candle is burning normally, with a fully formed cup, and a red nose to the wick. The apparatus I now publish enables an observer to comply with these indispensable conditions almost without trouble. The balance enables him to weigh the candle while burning, and the elevating piston keeps its flame in the proper position.



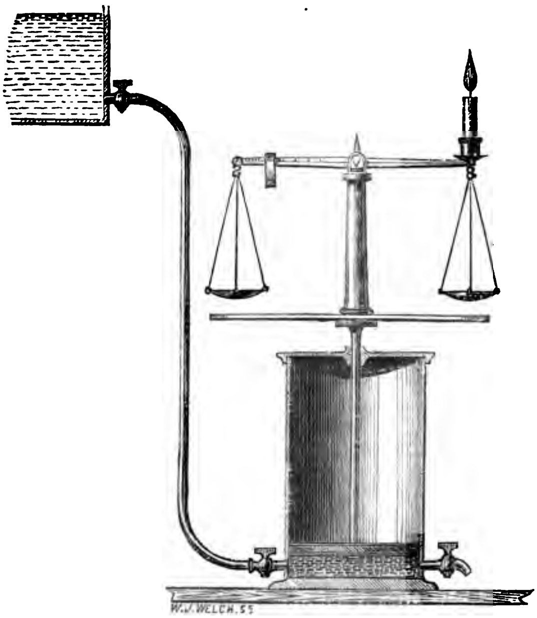
One end of the beam carries a candle-socket, and is furnished with a scale-pan having strong brass supports, which allow of the force required to thrust the candle or socket into its place without injuring the knife edges which carry the beam. A projecting ledge catches any sperm which may happen to run down. A movable counterbalance to the candle is placed on the arm of the balance, and supports are provided to keep the beam off the knife edges when the apparatus is not in use. The whole is covered by a movable case to keep off dust and damp.

To use the apparatus the candle is lighted, a 20 or

30-grain weight is placed in the scale-pan at the end opposite to the candle. The whole is counterbalanced, the weight removed, and the candle is allowed to burn till the balance is in equilibrium. By that time the candle will be burning normally. A 30 or 60-grain weight is then placed in the scale-pan at the candle end, and the time is accurately noted. A series of observations is taken in the usual way, and when so much sperm has been consumed that the balance is in equilibrium, the time is again noted. From the time taken to burn the quantity denoted by the weight placed in the scale, the quantity burnt per hour is calculated.

While burning, the candle is continually becoming shorter, and so removed from opposite the centre of the disc, if it be there at the commencement of the observations. To remedy this, the balance which carries the candle is fixed upon a rod, which is moved up and down by a piston placed in the cylinder. The lower part of the cylinder is connected by a pipe with an elevated cistern of water. By this pipe the water is conveyed beneath the piston, which it elevates on the principle of the hydrostatic paradox. The rate at which it is raised is made to correspond with the rate at which the candle burns. This rate is found by experiment, and is practically the same for all the candles of one lot purchased of a good maker. The water is allowed to drip from a tap in the cistern at a rate which is found by experiment to raise the piston as much as the candle diminishes by burning. This tap is not altered when once set, but at the end of each experiment the water is shut off by another tap. When a new candle is placed in the apparatus, the water contained by the cylinder is drawn off through a pipe provided for that purpose. The piston then descends, which brings the flame of the new candle opposite the centre of the disc, and wher is lighted, the tap from the cistern is allowed to drop, to keep the flame in that position. By this apparatus the object sought by cased candles is attained without the errors introduced by casing; it also obviates the very serious errors inevitable when a candle is weighed before and after use. It takes account of nothing but sperm consumed normally.

Any other kind of candle-balance may be placed upon my elevator, and if the candle be moved up each time it is used, so as to bring the flame opposite the centre of the disc when the piston is at the bottom of the cylinder, the latter need not be longer than will allow of about an hour and a half's consumption, instead of long enough to raise the candle through its whole length. In this case, the water must be drawn out of the cylinder each time it is used. A form of candle-balance which would be suitable for this purpose is shown below.



SUBSTITUTES FOR TEST CANDLES.

Several substitutes for test candles have been proposed, but none of them have been adopted, probably from the well-grounded conviction that the adoption of any of them would have been but a change of one error for another, and not the substitution of certainty for doubt; of truth for error.

One proposal was the adoption as a standard of a jet of olefiant gas, burning a prescribed quantity per hour, under prescribed conditions. The conditions which would secure accuracy could be prescribed easily, if pure olefiant gas were generally obtainable; but every chemist knows the great care and trouble required to produce it pure, and few would hesitate to affirm, that if it were used as a substitute for test candles, the results obtained would be worthless in most cases, owing to the impurities in the olefiant gas. If it could be obtained pure with ease and certainty by those who are not experienced chemists, it would afford an excellent substance for use as a standard light-measure.

It has also been proposed to use the carcel lamp in the place of sperm candles, as is done in France. The efficient use of this would demand an amount of personal skill rarely found in gas examiners, and would demand an amount of time which few Englishmen would expend upon it. Oils vary considerably in their illuminating power, according to the methods of their purification. Wicks vary much in quality of material and in their manufacture, and the difficulty of trimming a lamp-wick so as to secure a uniform light for the same weight of oil burnt throughout an entire year, is one which a person must be extremely clever or extremely reckless to encounter. For one who succeeded, dozens would fail, and no standard can be trustworthy which is thus dependent upon personal skill.

A NEW STANDARD LIGHT-MEASURE.

I have the pleasure to submit a new standard light-measure, which will be uniform everywhere, and which cannot fail to yield correct results, whenever it is employed in conformity with rules which can be given with ease and accuracy. If a Government commission were appointed to examine the whole question of a light-measure, I have no doubt that it would adopt the one I now proceed to describe, and would publish such directions as would render error impossible, except to the culpably careless, or the wilfully wrong-doing.

Carbonic oxide and hydrogen can be obtained pure without difficulty; and, therefore, when either of these gases is burnt at the same rate through the same burner, the heat produced and the light given (if any) will be the same also. I therefore take one of these gases to produce *heat.*

Naphthalin can be obtained pure without much trouble; and though such a thing as a pure sample of naphthalin cannot now be purchased in commerce, yet, if a demand were to spring up, I would undertake to have it supplied. Naphthalin being a definite chemical compound, and not a mixture like hydrocarbon oil, I take that to produce *light;* and by the combustion of a prescribed quantity of pure naphthalin per hour with a prescribed quantity of pure hydrogen or carbonic oxide per hour, in an apparatus of a particular form, which can be described in words and deposited in one or more standards, of which all others are to be exact copies, I make a fixed standard by which *all gas* made for sale shall be tested.

A general sketch of what is required to secure accuracy, is the following. The carburettor must be made exactly like the deposited model. It must be supplied before each experiment with a weighed quantity of naphthalin, and be weighed afterwards to ascertain that the proper quantity has been consumed. The temperature of the naphthalin must be uniform during the experiment. The gas supplied must be burnt at a uniform pressure and rate per hour, and the quantity consumed in each experiment must be measured. The light in this case can be made equal to that of the gas tested, if it were thought desirable, and thus one strong objection against candles would be done away. The materials and apparatus used would be invariable, and thus the errors arising from manufacture and personal manipulation are prevented, and the photometric process is rendered far more accurate and trustworthy than it is at present.

CHAPTER VI.

TEST BURNERS.

We possess satisfactory evidence that the burners in use before and at the passing of the various Gas Acts which established standards of illuminating power, were very different from those now used by gas companies to test gas, and yielded results far lower than can be obtained with the altered Argand. This is the same as saying that the burners now employed make the gas appear much better than it would have appeared if tested by the form of burner in use when the Acts passed, and therefore that the report of the examiner of a particular gas does not represent any advantage gained by the consumers of that gas, though it represents the gas as above the parliamentary standard.

Accum wrote on gas in 1819, and his work furnishes exact measures of the burners then in use, and of the size of flame obtained by a given consumption of gas. He writes: "The Argand burner (figs. 10 and 11, Plate V.) consists of two concentric brass tubes, about 1½ in. long, and *seven-eighths of an inch in diameter* (the largest size burner employed). The interval between the two tubes is closed at top and bottom. The upper part is closed with a ring of steel, which is perforated with fifteen or eighteen holes, $\frac{1}{10}$ of an inch in diameter. . . . . The height of the gas flame should never exceed three times the diameter of the flame" (pp. 253-4).

Description of burner.	Size of holes.	Diameter of perforations.	Height of flame.
15-hole Argand	·033 in.	·875 in.	2·625 in.

At p. 276 he explicitly reprobates large holes, saying, "If the holes be made larger than $\frac{1}{25}$ part of an inch (= ·04 each) in these kind of burners, the gas is not completely burnt." I do not quote Accum as an *authority*, but as a *witness* to what an Argand burner was in his time.

Peckston published the third edition of his book on gas in 1841, and he is a competent witness to what an Argand burner was then. P. 28: "It is generally admitted that an Argand burner of 15 holes will consume about 5 ft. of gas per hour, and yield a light equal to 12 mould candles of six to the pound." This sentence looks like the origin of the modern standard of 12 candles for a consumption of 5 ft. of gas per hour, with the difference of sperm for tallow. At p. 36 he writes: "In using Argand burners, however, the consumer should always do so with chimney glasses, and be careful that the flame should never on any account rise higher than from 3 in. to 3½ in. If it be allowed to rise higher, more gas will find its way through the burner than can be consumed, and consequently a portion of it will pass off in smoke, to the injury of his ceilings, or goods, or both." This appears to indicate that 5 ft. of gas in a 15-hole Argand burner gave a flame 3 in. to

$3\frac{1}{2}$ in. long. In p. 354 he gives a table of the several dimensions of the 15-hole Argand burner:

"Outer diameter of the top .	$1\frac{1}{16}$	of an inch	
Inner diameter of the top .	$\frac{19}{32}$	"	= ·593 in.
Diameter of the circle of holes	$\frac{3}{4}$	"	= ·75 in.
Diameter of each hole . . .	$\frac{1}{32}$	"	= ·0312 in.

"The holes drilled at the top of this burner are $\frac{3}{16}$ of an inch from centre to centre. The bottom of this burner is $1\frac{1}{8}$ in. diameter. The bottom is bell-mouthed, as shown in the figure. Height of this burner, 2 in. Distance from the top to the shoulder for supporting the gasholder, $1\frac{1}{4}$ in."

This is a description of a good burner, and I have no doubt of this being the kind of burner which was intended to be used to test gas when the Legislature fixed a standard of illuminating power. It is fit to burn well —not wastefully but thoroughly—12 to 14-candle gas, and it will be found to give a flame of about $3\frac{1}{2}$ in. to 4 in. long, with a consumption of 5 ft. an hour. I have seen the flame fork above the top of the 7-inch chimney when 5 ft. of 13-candle gas has been burnt through some of the modern 15-hole Argands used for testing.

Mr. Clegg, in his work on coal gas, 1853, is not specific in describing the standard Argand, but he says enough to show that what he so regarded differed but little, if at all, from the burner which I and others purchased for testing in 1860, during the passage of the Metropolis Gas Act through Parliament. He says (p. 277): "An Argand burner $\frac{3}{4}$ in. in diameter (the diameter given by Peckston) within the drilled ring, with a flame $2\frac{1}{4}$ in. high, consumes $3\frac{1}{2}$ cubic feet per hour."

At p. 278 he proves that the burner he used for testing gas would burn 6 ft. per hour, a quantity which *can* be burnt with burners constructed as they were in 1860, but which would give a column of smoke such as no person can endure, if an attempt were made to burn it in such 15-hole Argands as have actually been used of late to raise the apparent illuminating power of gas. Comparing Leslie's burner with the standard Argand, he says of the former: "The result is a less quantity of light with the same quantity and quality of gas than is due to the common Argand, as the following photometric results show:

Common Argand, 15 *holes; length of chamber*, $1\frac{7}{10}$ *inch.*

6 cubic feet of gas per hour, equal to . . 17·42 candles.
5 „ „ „ „ . . 13·64 candles.

Leslie's Burner, 28 *holes; length of tubes*, $1\frac{3}{8}$ *inch.*

6 cubic feet of gas per hour, equal to. . 14·73 candles.
5 „ „ „ „ . . 11·28 candles."

The consumption of 5 ft. of gas per hour, with an illuminating power of 13·64 candles, involves most offensive products of imperfect combustion when it is consumed in such a burner as Sugg's steatite Argand, now commonly used to test gas; and 6 *ft. an hour* could no more be burnt by that burner than a quart of fluid could be thrust into a pint measure.

I cannot refrain from observing, that, in 1827, Dr. Ure ("Dictionary of Chemistry," 3rd edition) quoted Accum's dimensions of burners with apparently the fullest confidence, and this could hardly have happened unless Accum's dimensions agreed with those of the burners which Dr. Ure was accustomed to use and to approve. Such a quotation, by so competent an experimentalist, fairly signifies approbation.

I think impartial persons will be convinced that from the years 1819 to 1853, the burner used to test gas was practically the same, and, what is of still greater importance, that it was *such a burner as the public were in the habit of using* to burn the gas tested. During that long period, the consumer obtained the light of 12 sperm candles by burning 5 ft. of gas per hour in a 15-hole Argand burner with a 7-inch chimney, when 12 candles were the standard of illuminating power found at the testing place.

We are able to carry on the evidence to the year when the Metropolis Gas Act was passed, and in this case the author is a gentleman well known in Parliamentary Committees, and, I believe, appearing for gas companies, seldom if ever against them: I refer to Dr. Frankland, who wrote the article on Coal Gas in Ure's Dictionary of Arts, 5th edition, 1860. At p. 733, vol. i., is a description of the Argand burner:

"The *Argand* consists of a hollow annulus (see fig. 482), from the upper surface of which the gas issues through a number of small apertures, which are made to vary in diameter from $\frac{1}{32}$nd of an inch to $\frac{1}{50}$th of an inch, according to the richness of the gas; the most highly illuminating gases requiring the smallest apertures. The distances of the orifices for coal gas should be ·16 to ·18 in., and for rich cannel gas ·13 in. If the Argand ring has 10 orifices, the diameter of the centre opening should be $\frac{4}{10}$ths of an inch; if 25 orifices, it should be 1 in. for coal gas; but for oil gas with 10 orifices, the central opening should have a diameter of $\frac{1}{2}$ an inch, and for 20 orifices, an inch. The pin-holes should be of equal size, otherwise the larger ones will cause smoke, as in an Argand flame with an uneven wick."

To compare these dimensions with others given, I have expressed them decimally:

Burner for common gas, size of holes = ·0312 inch
the very size given by Accum
Burner for cannel gas, size of holes = ·02 ”
10-holed Argand, internal diameter = ·4 ”
25 ” ” ” ” = 1·0 ”
15 ” ” ” ” = ·6 ”
in the same proportion
15 ” ” ” ” = ·75 ”
for oil gas

Mr. Leoni has made an excellent series of burners of what he calls "Adamas," an artificial siliceous compound. The dimensions of his perforations in Argand burners correspond nearly with those above given. In Argands for burning cannel gas, his perforations are ·015 in. for common coal gas (about 13-candle gas), they are ·031 in.; "and extra large for poor coal, wood, or peat gas," they are ·052 in. The burners with these very large perforations are fit to *burn* nothing but what they are proposed for, viz. "*poor* coal, wood, or peat gas," and yet they are but the minutest fraction larger than the perforations in the Birmingham burner, which is designed to consume 14-candle gas. Leoni's = ·052 in., Birmingham = ·05 in.!

In 1860, when the Metropolis Gas Act was before Parliament, I was anxious to procure a test burner, such as was then employed to test gas. I purchased one from a London firm, which had the highest reputation for the manufacture of testing apparatus, and whose customers were almost entirely gas companies. I am perfectly justified, therefore, in regarding this as a *typical burner* for gas testing in 1860, and in asserting that this burner is the one meant in the Metropolis Gas

Act, and in all Acts passed prior to that time. If a test burner had been demanded by the Parliamentary Committee, this kind of burner would have been produced and accepted by both sides, and I therefore contend that producer and consumer alike are bound by this, and that no other should be used till the question has been reconsidered, *and the authority which imposed the standard has altered its measure.* I can make a burner which will tell against the gas companies, and yet conform to the Act of Parliament. To use such a burner in testing would be a wilful fraud. On the other hand, I can make a burner which will tell against the consumers, and yet conform to the Act of Parliament. To use such a burner in testing gas would be a wilful fraud as great as the other. The only reasonable and fair mode of construing the Act which omits a definition of the test burner, is to take that which was commonly accepted by competent experimentalists at the time when the Act was passed. Should the question ever come to be settled by a court of law, I think that this would be the interpretation put upon the Act.

I have been thus particular on this point, because it involves large interests, and *may* involve grave moral questions. The burner purchased by me in 1860 is one which any consumer could use, and which represents exactly what the consumer receives at home when he burns the tested gas; that is to say, gas tested by it and called 12-candle gas, would give the light of 12 candles in an ordinary Argand in a house or shop. To illustrate this, I append a Table, prepared by me in 1864 for the Committee of the House of Lords which sat on the Bill of the Birmingham and Staffordshire Gas Company.

This Table led to the first attempt to define a standard burner for testing gas.

Different illuminating value of the same gas when burnt at the rate of 5 ft. an hour through various Argand burners, all of which have 15 holes each, and a 7 in. chimney.

The comparison is made with a normal 15-hole Argand, such as was in use when this kind of burner was taken as a standard.

	Size of holes.	Diameter of burner.	Central aperture.	Illuminating power.
1. Normal Argand iron	·0208	·87	·72	100
2. Sugg's steatite ...	·0416	·81	·43	114·5 Gain 14·5
3. Ditto with perforated disc..	...	...	..	117 Gain 17
4. Wright's iron ...	·0416	·84	·62	115 Gain 15
5. Leoni's Adamas with external cone	·0526	·87	·59	111 Gain 11
6. Platow's iron double cone..	·0208	·90	·56	89 *Loss* 11
7. Normal Argand with outer glass	same as No. 1	...	...	156 Gain 56

These results are sufficiently startling, but they are fully corroborated by gentlemen acting in the interests of the gas companies; by Dr. Letheby, who appeared as a witness for the Birmingham and Staffordshire Gas Company, and by Mr. Rutter, of Brighton, whose well-timed writings have done much to further the general use of gas, and so to advance the interests of gas companies.

Fairness requires the whole subject to be placed before the reader in various aspects, and therefore I append Dr. Letheby's table of burners, together with the reported observations by which it is explained. He said (Lords' Committee, April 28, 1864):

"I am well acquainted with everything which led to the Metropolis Gas Act of 1860. I was constantly engaged for eight years before the passing of that Act in the examination of burners. There were different kinds of burners in use when the Act was passed, but it was not so well known then that such enormous differences existed in the value of burners supplied by different persons. None of the burners lying on the table were in use at that time, but Sugg's came into practical use about that time. I have recently tested two brass burners supplied by makers of instruments for photometrical experiments—one at Birmingham at the old company's works, and the other at Nottingham, and there was a difference of 10 per cent. in the illuminating power; so that had the worst of the two burners been used to test gas about the standard quality, the company would have been unjustly charged with supplying gas of an inferior quality. I am gas analyst for the City of London. The range in the different varieties of burners used is at best (least?) 35 per cent. The following are the results of testing the illuminating power of common London gas with different burners which I have obtained:—

Kind of burner.	Diameter of internal aperture.	Illuminating power in standard sperm candles.	Per-centage of difference of value.
Old brass burner, 15 hole and 7-in. chimney	0·70	11·30	100
Old brown burner...............	0·57	12·12	108
Steatite burner	0·50	13·23	117
„ „ with gauze diaphragms	...	13·04	115
Steatite burner, no gauze......	0·44*	13·56	120
with gauze...	...	13·35	118
„ „ no gauze......	0·43	13·00	115
„ „ with gauze...	...	13·56	120
„ „ no gauze......	0·42	12·50	110
„ „ with gauze...	...	13·09	116
Porcelain Bengel Paris burner with 8-in. chimney ...	0·35	15·33	135

"When testing the illuminating power of the gas supplied in London, I use a burner which has an internal aperture of $\frac{44}{100}$ths (*i.e.* the burner marked with an asterisk in the Table), and the burner is used without gauze round it. I do that simply because I find it gives the highest illuminating power. A burner without gauze, having an internal diameter of ·44, gives the illuminating power of the London gas at 13·56; whereas, by using a burner supplied by a photometrical instrument maker, the same gas would only show an illuminating power of 11 or 10. My impression, which is founded on very great experience, is, that, as the burner does not give the light, the companies are entitled to that burner which will give the highest illuminating power."

This Table fully bears out all I have said, and the explanatory observations by which it is accompanied are so clear, that they need no elucidation. I regret, however, that the size of the holes and the diameter of the ring of holes is not given. This would have enhanced the value of the Table.

Dr. Frankland, professor of chemistry at the Royal Institution, Albemarle-street:

"I agree generally with Dr. Letheby's evidence. I concur with him as to the burners in use in 1860. There were a variety of burners in use at the time of the passing of the Metropolis Gas Act. The burner contemplated was an Argand burner, with *an aperture of more than 6-tenths of an inch in diameter*, with 15 holes, and a 7-in. chimney."

Dr. Frankland concurs with every one conversant with the subject in stating that the burner intended by Parliament was one which gave a much lower illuminating power than burners now used in photometry—in fact, such a burner as consumers used then and use now.

Mr. Rutter, of Brighton, is well known by a number of little books which have done much to extend and popularise the use of gas. Gas companies owe him a great debt of gratitude. In September, 1864, this gentleman published a long letter on "The Photometry of Gas-lighting, its Failures and Successes." The letter has intrinsic merits which command attention, in addition to its being the production of a veteran who has laboured usefully. He writes: "In the struggle of gas legislation, nothing appears easier or pleasanter than discussions about the standard of illuminating power; sperm candles consuming 120 grains per hour; Argand burners with 15 holes and 7-in. chimneys; 5 cubic feet of gas per hour, and 5-tenths of an inch pressure; and then subject to these conditions a deposited burner. Will any one acquainted with the science of gas-lighting, who has had some practice in photometry, and who knows what great differences in illuminating power are produced by small differences in the forms and dimensions of Argand burners, say that the foregoing conditions ought to be considered satisfactory?

"Here are a few suggestions. Beginning with a standard Argand burner, how should it be described? Perhaps somewhat in this way, namely (and, by way of example, let it be the Birmingham burner):

		Inches.
1.	External diameter at base	1·24
2.	„ „ at top	1·10
3.	Internal „ base	0·68
4.	„ „ top	0·50
5.	Height of burner	2·9
6.	„ from centre of crutch	1·82
7.	„ of body	1·50
8.	Number of holes	15
9.	Size of do.	0·05
10.	Internal diameter of circle	0·74
11.	Surface of burner	(level)
12.	External diameter of gallery	2·30
13.	Internal do.	2·00
14.	Number of bearing bars, 4	
15.	Width of do.	0·11
16.	Depth of do.	0·13
17.	External diameter of ring	1·24
18.	Height of cylindrical chimney	7·00
19.	Internal diameter of do.	2·00
20.	Thickness of do.	0·06
21.	Material of which the burner is made (brass)	

"Admitting as the right principle that gas companies are entitled to any particular form and size of burner best adapted for the qualities of their gases, and by which, other conditions being equal, the maximum illuminating power is produced,* I yet think that practical utility should not be so entirely ignored. So completely has there been a passing from one extreme to another, that it must have been noticed how ill-adapted test burners are for producing what used formerly to be so greatly admired—a symmetrical gas flame.

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* No one but a company's advocate would admit anything of the kind. The draper has the same right to select the length of the yard by which he measures, and the grocer the weight of the pound by which he weighs.

"If burners are well made and properly managed, those used only for testing and experimental purposes are remarkably durable. *Five 15-hole London Argands* (*as they used to be called*), which have been in my possession thirty-one years, and have been used many hundreds of times, are as perfect as when they were new."

With the intention of illustrating some of the recent changes, and of showing where and to what extent alterations have been made, I here quote the dimensions of a few burners, the parts being indicated by the numbers as given below:

A. One of the old burners, just now mentioned.

B. A similar burner, lately obtained from one of the best makers, *which is commonly used by the public.*

C. Another, by the same maker, slightly altered, and with larger holes.

D. A test burner.

E. The Birmingham burner, repeated for convenience of reference.

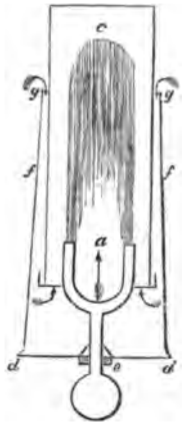
Nos.	A	B	C	D	E	
1	1·22	1·22	1·22	1·22	1·24	
2	1·07	1·08	1·08	1·08	1·10	
3	0·66 ?	0·65 ?	0·54	0·63 ?	0·68 ?	
4	0·62	0·54	0·54	0·46	0·50	
5	3·00	2·90	2·90	2·90	2·90	
6	1·85	1·85	1·85	1·90	1·82	
7	1·60	1·46	1·50	1·50	1·50	
8	15	15	15	15	15	
9	0·04	0·04	0·06	0·06	0·05	
10	0·78	0·80	0·74	0·75	0·74	
11	Level.	Level.	Level.	Convex.	Level.	
12	2·30	2·30	2·20	2·30	2·30	
13	2·10	2·10	1·90	2·10	2·00	
14	4	4	4	4	4	
15	0·13	0·12	0·12	0·12	0·11	
16	0·12	0·13	0·13	0·14	0·13	
17	1·22	1·24	1·22	1·23	1·24	
18	...	...	...	...	...	
19	...	...	...	...	...	
20	...	...	...	...	...	
21	Brass with iron top.	Brass with iron top.	Brass with iron top.	Brass with steatite top.	Brass.	
	100	104	112	121	113	Illuminating power with one kind of gas.
	100	100	114	116	117	Illuminating power with another kind of gas.

What are the effects of diminishing the internal diameters and of enlarging the holes of Argand burners? Here are examples of some of the most commonly occurring phenomena, showing that, other conditions being equal, *the light is thereby increased:*

Rate of consumption = 5 cubic feet an hour.
Pressure = 5-tenths.
Chimneys = 7 inches.
Old Argand burner A reckoned as = 1·000
A = 1·000. With gas of a different quality = 1·000
B = 1·042 . . . . = 1·008
C = 1·125 . . . . = 1·143
D = 1·217 . . . . = 1·169
E = 1·130 . . . . = 1·171

"Taking the Birmingham burner as a type (rather exaggerated, perhaps) of recently made test burners, the difference between that and the old Argand is almost as great as it can be; the flame from the first being yellowish and tapering and about 5 in. high, and from t other intensely white and cylindrical and about 3 in. high. The Birmingham burner yields the greatest quantity of light; the greater area of the luminous surface being more than an equivalent for defectiveness of colour."

"A test burner, for obvious reasons, must stand alone; *being wholly inapplicable to any of the ordinary purposes of gas-lighting.*"



No. 7 in my Table, p. 113, needs an explanation. It refers to a mode of burning gas introduced by me in January, 1854, and figured here. By surrounding the Argand burner and chimney with another glass cylinder, I diminished the force of the air current and the cooling effect of the air upon the flame, and by thus raising the carbon to a higher temperature, and keeping it in the flame for a longer time, I obtained an increase of light from the same gas of 56 per cent. This result is not mentioned by Dr. Letheby and Mr. Rutter in their Tables, but it is more than confirmed by Dr. Frankland, who wrote the article on Coal Gas in the last edition of Ure's Dictionary of Arts. He experimented with my apparatus, and gave a figure of it in the article, and states that he obtained with it an increase "of 67 per cent. of light for equal consumptions." This is more than corroboration of my result marked No. 7. It is thus shown that, at the will of the person testing, the same gas may be represented to have a value of 89 only, or of 156; and yet, whether reported by the one value or the other, it is unaltered to the consumer. I have expressed the opinion that this discrepancy of reporting would not be tolerated by a court of law. I am bound, however, in fairness to add, that very eminent counsel are of a contrary opinion, and maintain that, inasmuch as all the burners satisfy the *words* of the Act of Parliament, any one may be selected at the will of the tester. If this be fact, the law should be altered without delay.

In consequence of what took place before the Lords' Committee on the Birmingham and Staffordshire Gas Bill, an attempt was made to fix a standard burner for testing this company's gas. The Committee could not have been much more favourable to the company if they had been its shareholders. They selected a burner which gave the company a great advantage in making poor gas look good, and at the same time raised their standard of illuminating power. This gave with one hand and took away with the other. The measurements of the burner were left to myself and Dr. Letheby, and, as adopted by the Lords' Committee:

The description of the burner was, that it was to be a brass burner, having an external diameter of 1·1 in., an internal diameter of ·5 in., and having 15 holes, each hole of the diameter of ·05 in. To be used with a 7-in. chimney, without a perforated disc.

To show the fallacy of adopting a burner such as this, with a loosely-worded definition, I suggested three different burners, all of which satisfied the words of the Act as it left the House of Lords, and all of which gave different results: *i.e.* represented the *same gas* as of a different value.

No. 1 was drilled at the *outer circumference* of the flat steel top.

No. 2 was drilled at the *centre* of the steel ring.

No. 3 was drilled at the *inner circumference* of the ring.

The dimensions in every other respect were the same in all three burners.

These burners were tested with gas of the illuminating power of 13·4 candles. The testings were made a same time, with the same instruments, as well as with the same gas, so that all possibility of error might be avoided.

No. 1 burnt 5·1 ft. per hour, with a flame but little above the glass.

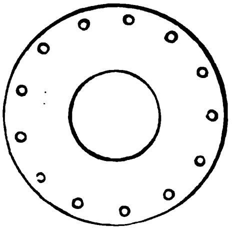
No. 2 burnt 4·86 ft. per hour, with a flame 3½ in. above the glass.

No. 3 burnt 3·86 ft. per hour, and yet with this small quantity the flame had long smoky tails above the glass.

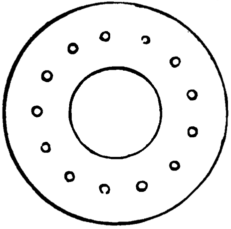
Neither of these burners, consuming the above quantities, could have been endured in a room, owing to the escape from the flame of unconsumed gas.

The mode of drilling the burners and the kind of flames they produced are here shown:

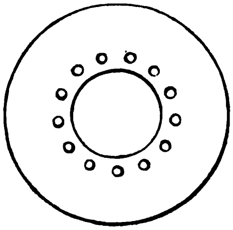
No. 1. drilled at the outer circumference of the steel top.



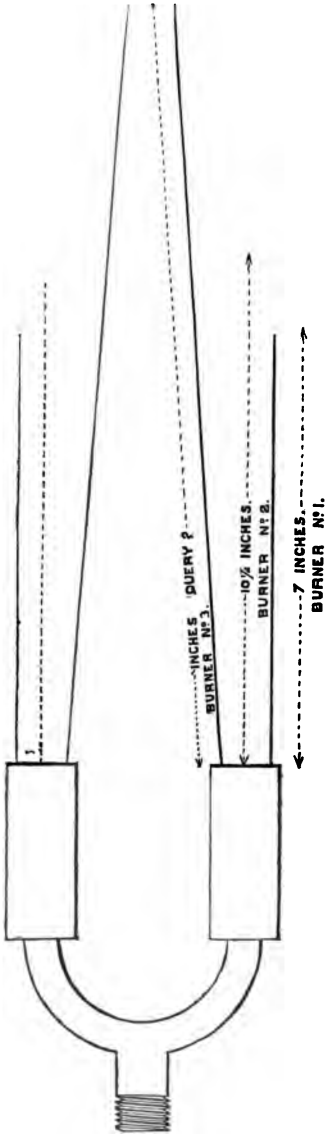
No. 2 drilled at the centre of the steel top.



No. 3 drilled at the inner circumference of the steel top.



The lines in the annexed cut represent sections of the flames, and the figures express their heights.



These experiments showed that the words of the Act were useless for the purpose of fixing a standard burner; and, moreover, that the burner made like No. 2, as was doubtless intended, was not fitted to burn 5 ft. an hour of gas having an illuminating power of 13·4 candles properly determined. It was fit to make 12-candle gas appear 14-candle gas, but it was wholly unfit to determine the illuminating power of what was meant by 14-candle gas in 1860, and is sold as such in many places within my own knowledge.

To show the influence of a test-burner in making *bad* gas appear *good*, I mixed atmospheric air with gas in a gasholder until its illuminating power was reduced from 13·4 candles, when burnt in a standard Argand, to 11·45 candles, when burnt in Wright's burner (see No. 4, in Table at p. 113). With gas thus deteriorated, I obtained the following differing values for 5 ft. an hour, with the following burners and 7-in. chimneys:

1.	Wright's Argand, 5 ft. an hour, equal	11·45 candles.
2.	Sugg's Argand	11·87 „
3.	Birmingham and Staffordshire, perforated at the inner circumference of the ring	13·3 „
4.	Birmingham and Staffordshire, perforated at the centre of the ring	11·45 „
5.	Birmingham and Staffordshire, perforated at the exterior of the ring	8·75 „
6.	Standard Argand	7·3 „

The burners in the experiments were used in the following order: 1. Wright's; 2. Sugg's; 3. Standard Argand; 4. Birmingham and Staffordshire exterior perforations; 5. Birmingham and Staffordshire centre perforations; 6. Birmingham and Staffordshire interior perforations; and at the close, to prove that the air had mixed perfectly with the gas, I determined the illuminating power of the mixture by Leoni's Argand, and found it within a small fraction of 12 candles.

These experiments are most suggestive. They show that a large quantity of air may be mixed with gas so as most seriously to lessen its value to the consumer who burns it in *ordinary* Argand or flat-flame burners; and yet the illuminating power *at the photometer* may hardly be impaired, provided a burner be used in testing which will so regulate the air current as to get light out of the deteriorated mixture. The light of the unadulterated gas, determined in the usual manner, equalled 13·4 candles. The light of the adulterated gas equalled 13·3 candles, when consumed in a burner adapted to burn the mixture, but not adapted to burn gas. I do not say that a mixture of gas and air should not be sold, nor that a burner constructed as mine is should not be used to burn the mixture; but I do say that the *mixture* should not be sold for *gas*, and that the definition of a standard burner for testing should not be so left as to compel a gas examiner to report gas above the required illuminating power, when that gas, as burnt by those who purchase it, gives but little more than half the light it ought to give.

It will probably be urged that a mixture of air and gas was quite unnecessary to try these burners, as gas companies do not mix air with their gas. I do not affirm that this is done intentionally to a considerable extent; but I do affirm that the exhauster introduces more or less air to all gas when it is worked so as not to leave some pressure upon the retorts, and when clay retorts are leaky; and that sometimes the quantity of air in gas is so large as seriously to interfere with its illuminating power when burnt through ordinary consumers' burners, especially flat-flame burners. More than once it has happened that the Houses of Parliament have been reduced to a state of darkness visible by a rapid falling off of the light-giving power of the gas, and upon inquiry it has turned out that the gas was damaged by the presence of air. The exhauster at the gasworks had been allowed to run too fast, and had pumped air into the gasholder as well as gas. The gas burnt in the Houses of Parliament is cannel gas, and consequently the burners employed are flat-flame burners, and have fine apertures. The loss of light by the presence of air in gas is far more considerable when the mixture is burnt in thin than when it is burnt in thick streams from burners of large aperture. I subjoin what has been published in reference to the effect of air upon the illuminating power of gas. It is unfortunate that Dr. Letheby does not inform us whether his table is intended to give the results of his own experiments, or of those of Messrs. Audouin and Bérard; and if they were his own, the kind of burner used and gas experimented with. The French experimentalists state distinctly that their results were obtained by using a bat's-wing burner with a slit $\frac{1}{16}$ of an inch in width.

AIR IN GAS.

The following observations upon the influence of air in gas were addressed by Dr. Letheby to a meeting of gas engineers at Birmingham last year:

OXYGEN.—" This comes from the too active operation of the exhauster, and the drawing in of air through the retorts. The mischievous influence of this gas on the illuminating power of coal gas will be seen from this experiment, where I will pass about 6 per cent. of air into the burning gas, and you will notice how seriously the light of the flame is destroyed. It has fallen, in fact, to about half its original power. The following are the proportions of light lost by different amounts of air in gas, supposing the light to be 100:

	Light.
2 per cent. of air in gas	90
5 „ „	70
7 „ „	52
10 „ „	34
20 „ „	12
40 „ „	1
50 „ „	0

(*Chemical News*, vol. xii., p. 18.)

There is an obvious error here in attributing the whole effect of the air upon the light-giving power of the gas to its *oxygen*, to the entire exclusion of its other constituent, nitrogen. I quote the paragraph as showing the influence of atmospheric air, not of oxygen.

Mr. Richards has very recently published a statement which differs widely from the above. He says: "According to Messrs. Audouin and Bérard, 6 parts of air intermixed with 94 parts of gas reduce the illuminating power to one-half of the light of the same gas when pure; and when 20 parts of air are intermixed with 80 parts of gas, the illuminating power of the gas is entirely destroyed."

The following are the results of Messrs. Audouin and Bérard. They were obtained by burning the mixture of gas and air at the rate of 4·94 cubic feet an hour, through a bat's-wing burner, with a slit $\frac{1}{16}$ of an inch in width. Light of unadulterated gas = 100.

MIXTURE OF GAS AND AIR.

Quantity of air per cent.	Light.	Light lost by the addition of air.	Destructive power of each per-centage of air.
1	94	6	6
2	89	11	5·5
3	82	18	6
4	74	26	6·5
5	67	33	6·6
6	56	44	7·33
7	47	53	7·57
8	42	58	7·25
9	36	64	7·11
10	33	67	6·7
15	20	80	5·33
20	7	93	4·65
30	2	98	3·26
40	1	99	2·47
45	0	100	
50	0	100	

I have added the fourth line of this table to facilitate its practical use.

"It will be seen by this table that the introduction into lighting gas of 6 to 7 per cent. of air is sufficient to diminish its illuminating power one-half. A mixture composed of 20 per cent. of air and 80 per cent. of gas gives no light."

The author of the "Chemistry of Artificial Light," in "Orr's Circle of the Sciences," furnishes a valuable, because unintentional, proof of what a 15-hole Argand was in 1860, when the book was published, and when the Metropolis Gas Act was passed. Speaking of the *Argand burner*, he says: "The number of holes, or jets, varies from 10 to 30 for ordinary gas, and from 30 to 90 for cannel. *In the former case the holes are comparatively large*, and *in the latter they are very small.* For ordinary London gas, a burner with 15 holes and a 7-in. chimney is considered to be the best. Such a burner will consume about 5 cubic feet of gas per hour, and will give the light of 15 sperm candles." No measurements are given here, and all seems as indefinite as it well can be, but we shall be able to supply what is lacking nearly as well as if it were given.

Speaking of *Leslie's burner*, he says: "These burners are very well suited for the combustion of cannel gas, but they destroy the light of common gas by overburning it."

In a subsequent portion of the same chapter, there is a table of the relative illuminating value of the same gas when burnt through different burners, and among them are Leslie's and the 15-hole Argand:

Burner.	Cannel gas. Candles per foot.	Common gas. Candles per foot.
Leslie, with 28 tubes	3·6	2·7
Common Argand, 15 holes	4·0	2·8

Expressed in the usual way of the value in candles for a consumption of 5 ft. per hour, this is:

	Candles.	Candles.
Leslie	18·0	13·5
Argand	20·0	14·0

It is thus seen that cannel gas burnt in the 15-hole Argand used in 1860 gave 10 per cent. more light than when burnt in a Leslie's burner; and that common gas in the Argand gave 3½ per cent. more light than in the Leslie. This will enable competent people to define the Argand burner almost as correctly as if its dimensions had been figured.

At the outset, this excludes all the burners which have been introduced during the last three or four years for testing gas. Dr. Letheby says, very truly, in his evidence, that 5 ft. of even 15-candle gas will blaze out at the top of the 7-in. chimney when burned in the Birmingham burner, and I quite believe that 5 ft. of

20-candle gas would give a long smoky flame more than a foot above the glass. In fact, the gas would be wasted, not burnt. The steatite burner is still less fit to burn 20-candle gas, for its holes are larger and its central aperture is less, and it would therefore be more intolerable in use. The Argand burner of 1860 is one which *will* burn 20-candle gas, and show its light at the photometer, and yet will burn 14-candle gas, and give within 4 per cent. of the light of that gas when burnt in a Leslie's burner. The normal Argand which I purchased in 1860 will do this, as also will the burners mentioned by Mr. Rutter, as being used by him prior to the introductions of the last three or four years, and, let me add, as also will ordinary 15-hole Argands, which are purchased as articles of commerce in London, and used by the public. To be certain of the latter fact, I procured a 15-hole Argand from Hulett's, in Holborn, in 1864, and one from Strode's, in St. Martin's-le-Grand, in 1865, and both these burners will burn 20-candle gas, and will give nearly the same result with common gas as Leslie's.

Strode's is the better burner, and would be a perfectly fair instrument to use as between producer and consumer under the Metropolis Gas Act. I have ascertained that exactly similar burners are sold by Faraday, and by all respectable London gas-fitters, and *that these burners have been sold by them for many years past.* In every case where gas is tested under the provisions of an Act of Parliament, and no burner is specifically defined in the Act, these burners only ought to be employed.

"*The companies are entitled to the best means of educing the light of the gas.*" I will test this statement by a practical application. I am supplied by a gas company regulated by an Act of Parliament passed in 1846. To secure the company's capital, and to give them an opportunity to make a good dividend, Parliament gave them a monopoly of gas, thus preventing myself and others from making and selling gas. It took from us consumers the right which we had of making and selling gas, and obtaining profit thereby, and gave our rights to the gas company. I do not complain of this. On the contrary, I think the arrangement a good one. But when it took away our rights and gave them to others, it endeavoured to secure us from unjust treatment by the company, and therefore fixed the price, and the quality of the gas which they should be bound to supply us. The quality was such that 5 ft. of gas burnt in an hour through an Argand burner having 15 holes, and a 7-in. chimney should give *at least* as much light as 12 sperm candles of six to a pound, each candle burning 120 grains of sperm per hour. This standard of quality took effect directly the Act passed, and the next day after the Act became law the measure of quality between buyer and seller was a 15-hole Argand burner, giving the light of 12 sperm candles for a consumption of 5 cubic feet an hour. How can we learn in 1865, amid the confusion with which interested parties have endeavoured to surround the question, what kind of an Argand Parliament intended? We can learn it thus. Suppose the gas company had been prosecuted in 1846 for supplying gas which was below the proper quality, and so a fraud on the consumers, how would the judges in the case have ascertained the fact, so as to adjudicate? Would they not have required the tester to employ an ordinary 15-hole Argand, such as was in common use? The very fact that no definition was attempted, proves that on both sides the burner in common use was understood to be intended—or else the Parliamentary Committee would have imposed a defined instrument—and the judges would have ordered the person who tested the gas to determine the illuminating power by that. They must have done this, or have dismissed the case from inability to discover what was meant by a 15-hole Argand burner. They could not have done the latter, for a 15-hole Argand burner was commonly known, and had been frequently described by writers on gas, and the dimensions thereof had been given in detail. This would have been held to explain what the Act described, and this alone would have been used.

Was this intended to be a *fixed* standard or a *variable* one? *i.e.* had the company a right to alter the standard for its own advantage, and to the disadvantage of its customers, of its own mere will? In 1853, I showed the directors of the company how an increase of 56 per cent. of light could be educed from their gas without in any way altering the burner, or doing anything but surround the 7-in. chimney with another glass, the air to feed the flame being made to pass through the annular space between the glasses. Now in this case nothing was put into the gas, no alteration was made in the prescribed testing apparatus, but the light was educed from the gas by the application of a well-known law in science. Were the company at liberty to reduce the quality of their gas, and then make its quality appear "standard," by the new means of "educing the light?" If so, the Act of Parliament is something worse than a delusion: it is a legal protection of fraud.

I contend that the burner which was known, and would have been used, in a trial relative to the illuminating value of this company's gas in 1846, is the proper standard burner for ascertaining the illuminating value of its gas, so long as the Act of Parliament remains in force, and that it would be a fraud to use improvements in burners—unthought of when the standard was fixed—as the measurers of illuminating value. Just so in the Metropolis Gas Act, and in all other Acts prior to the Birmingham and Staffordshire Gas Act of 1864, the burner intended was the old burner which had been in use so long by experimenters, which I call a standard Argand, and which Mr. Rutter admits the gas companies, till very recently, regarded as a standard.

Strong as the known facts were prior to the last session of Parliament, they were all eclipsed by a statement of Mr. Hawksley, made before the Committee which sat on the Hanley Gas Consumers' Bill. Mr. Hawksley was called as a witness for the British Gas Company, and he made the following statement: "I have here a remarkable instance of a burner conforming to the ordinary description of a parliamentary burner, and this burner [producing one] is a 15-hole Argand burner—the ordinary kind of burner prescribed in most Acts of Parliament—and this burner is so badly proportioned, *that although it conforms to the Act of Parliament yet with* 12-*candle gas it gives the light of only three candles!!* There is another burner [producing same] conforming also to the Act of Parliament, but better proportioned for the burning of gas; this burner will develop the light of 14 candles, and will show it to be 14 candles, while the other burner, with a really 12-candle gas, will show but three candles' light."

This 3-candle burner conforms to the Act of Parliament, and therefore might be used to test gas with, and if it were so used a court of law must convict daily, if daily penalties were sued for, on testings made with this burner. Such a course would ruin a gas company, and, in my opinion, would be grossly dishonest; but it would be no more dishonest than using a burner which tells for a company and against consumers. One gives results abnormally high, and the other abnormally low, and every argument used in favour of the one may be used, *mutatis mutandis*, in favour of the other. The only present way out of the difficulty is to take such burners for gas-testing as were actually in use when the standards of illuminating power were fixed, and, for the future, Parliament should define the burner, as it has already defined the yard measure and the pound weight. The interests involved are too large to be left to caprice or dishonesty.

CHAPTER VII.

TESTING GAS FOR SULPHUR.

SULPHUR is known to exist in gas in several forms of combination. It may be there as sulphuretted hydrogen, as a sulphide of ammonium, as a sulphocyanide, as bisulphide of carbon, or as one of the substances which form what we call sulphuretted hydrocarbons, for want of better knowledge, which would enable us to name them accurately. These substances are tested for in various ways:

1. By lead-paper, which will detect sulphuretted hydrogen and sulphide of ammonium.

2. By burning the gas in an atmosphere of ammonia, and condensing the products of combustion. This will detect sulphur in any of its combinations, and will enable us to determine its quantity, but it will not afford any information as to its mode of combination in the gas burnt.

3. By passing gas through heated lime, and then through acetate of lead, alkalis, lime, &c. This shows sulphur if present, but does not enable us to determine its mode of combination.

4. By blowing the flame of a gas blowpipe upon the

surface of water, and precipitating the sulphur as sulphate of baryta. This does nothing more than indicate the presence of sulphur.

5. By passing gas through ammoniacal alcohol, and precipitating sulphur as a lead salt by acetate of lead. This indicates the presence of sulphur, nothing more.

6. By separating sulphuretted hydrocarbons and distilling them, or dissolving them in ammoniacal alcohol, and testing for sulphur by acetate of lead. An ultimate analysis would indicate the quantity of sulphur in the gas combined with hydrocarbons, and this, subtracted from the total sulphur, would enable us to approximate, more closely than we have hitherto been able to do, to the quantity existing as bisulphide of carbon, or other sulphur compounds.

7. By dissolving the bisulphide of carbon in hot alcohol, and precipitating the bisulphide of carbon by the addition of water. This is my own test, by which the presence of bisulphide of carbon in gas was first *proved.* Up to the time of my experiments its existence in gas was mere inference or probable conjecture. This indicates the presence, but not the quantity, of bisulphide of carbon in the gas.

8. By dissolving out the bisulphide of carbon by alcohol or ether, which contains triethylphosphine, and obtaining the bisulphide of carbon as a ruby-coloured crystalline salt with triethylphosphine. This indicates the presence of bisulphide of carbon only; it does not give its quantity.

I will explain each mode of testing.

1. *Lead-paper.*—This is made by soaking white filtering paper in a solution of basic acetate of lead, drying the saturated paper, and cutting it into slips for use. To make it, cut sheets of the best white filtering paper into

pieces about six inches square. Pour some basic acetate of lead (the "*Goulard water*" sold by druggists will do) into a clean plate, and lay the pieces of paper in the fluid, one by one. As each piece becomes saturated, take it out and hang it to dry upon a string in a place where no sulphuretted hydrogen can reach it. When dry, cut it into narrow slips, and keep these slips in well-corked bottles. The paper is ready for use at any time. Some persons damp it before use.

When lead-paper is used to test gas, a strip about half an inch in width should be slightly crumpled, so that it will not lie flat when placed upon a table, and put into a piece of glass tube about three-eighths of an inch in diameter, and four or five inches in length. This glass tube should be joined by short india-rubber tubes, or by union-joints with a gaspipe, and a stream of gas should be allowed to pass through the tube for not less than half an hour. A similar piece of paper in a similar tube should be compared with the test piece at the end of the experiment, and if the gas be free from sulphides, both will have the same colour. If, on the other hand, it contains sulphides, the test-paper will be darkened.

In gasworks, a continuous stream of gas should pass day and night over lead-paper in a tube, and be examined frequently. Some persons may be surprised at the length of time which I have given for this experiment. If a test is to be real and not delusive, the gas must *be brought into contact* with the substances which it is to affect, and must be *kept in contact* with them long enough to show a reaction, if impurity be present. There is very little gas sold which will bear to be tested by lead-paper thus properly applied.

I think it right to caution the reader against the common but erroneous inference, that coloration of lead-paper is necessarily due to sulphuretted hydrogen. I have met with another compound, both in and out of gas, which colours lead paper, and which is *not* sulphuretted hydrogen. It is a sulphur compound, and should be taken out of the gas; but we have conclusive evidence that it is not sulphuretted hydrogen. I never knew a trace of sulphuretted hydrogen to pass a large excess of clean peroxide of iron, but I have seen this sulphur compound pass through two feet of perfectly clean oxide of iron in a glass tube, and after its passage discolour lead-paper. We have a test for alkaline sulphides so delicate, that if a fragment of a human hair be dissolved in potash, the test will show the presence of sulphur derived from the hair: I refer to the nitro-prusside of sodium. I have passed this sulphur compound into nitro-prusside of sodium for some time, and have failed to obtain any indication of the presence of a sulphide, and yet lead-paper has been coloured. I think I can name the compound, and, indeed, I have done so to friends in private; but as I am not prepared here to give proofs of the accuracy of the conjecture, I forbear. It is right to put the reader on his guard against confounding this compound with sulphuretted hydrogen. As this compound may have interest for some readers, I extract a short account of it from a paper of mine presented to the Royal Society in 1862:

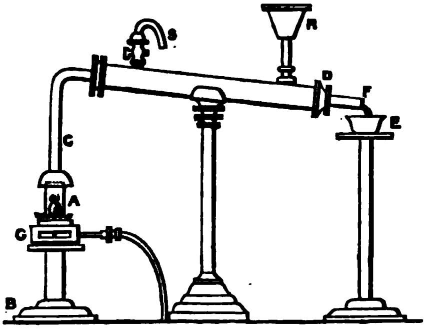
" The erroneous view of the action of heated hydrate of lime upon the sulphur compounds in gas (viz. that bisulphide of carbon reacted with the elements of water and formed sulphuretted hydrogen and carbonic acid: $C S_2 + 2 H O = 2 H S + C O_2$) arose, I think, from the generally-received opinion that the blackening of lead-salts by a gas is a proof that that gas is sulphuretted hydrogen, joined to the fact that sulphuretted hydrogen is one of the compounds produced by the action of the heated lime. If the sulphuretted hydrogen had been separated from the mixture of gaseous compounds produced, the truth would have been apparent; but as I believe all experimenters have failed to separate them, the subject was obscure. After having failed in many processes devised by myself and suggested by others, I at last removed the sulphuretted hydrogen, and showed that the blackening of lead-salts is no proof of the presence of sulphuretted hydrogen. Ordinary purified gas was passed over heated hydrate of lime, then through a considerable quantity of well-washed hydrated peroxide of iron, over lead-paper, and subsequently through moist slaked lime. The peroxide of iron was slightly blackened, and withdrew every trace of sulphuretted hydrogen; the lead-paper became black, and the slaked lime yellow. This yellow lime gave a primrose-coloured solution with water, which precipitated lead and silver salts brownish red, thus showing the presence of impure hydrosulpho-carbonate of lime. To be certain of the absence of sulphuretted hydrogen, some of the yellow lime was treated with hydrochloric acid, and the gases evolved thereby were conducted into a solution of potash. The potash solution gave no reaction with nitroprusside of sodium, showing the absence of sulphuretted hydrogen, and when boiled with nitric acid, gave no precipitate with a salt of baryta.

" The hydrochloric solution of the lime contained a sulphur salt, which was obtained as sulphate of lime when nitric acid was added, and the whole was boiled.

"The blackening of the lead-paper in this case could not be due to sulphuretted hydrogen, for that compound was absent. Nor, I think, is it due to the hydrosulpho-carbonic acid which passed over and in contact with the lead-paper, and was arrested by the lime. The red compound which this acid produces with lead-salts is said to turn black rapidly; and the red compound produced by a lead-salt, and those of Berzelius and Zeise, undoubtedly does so blacken, as also does that produced by a salt of lead and an alkaline pentasulphide. I have, however, obtained a red-lead salt by the reaction of crystallised hydrosulpho-carbonate of lime and basic acetate of lead, which remains red after drying in the air at ordinary temperatures, and exposure for weeks to the free atmosphere. I therefore conclude that the blackening of the lead-paper in the above case was not due to hydrosulpho-carbonic acid, but to some unknown or unsuspected compound."

2. *Testing gas by burning it in an atmosphere of ammonia.*—When gas containing sulphur is burnt, the sulphur present becomes oxidised and forms sulphurous acid. Sixteen grains of sulphur combine with sixteen grains of oxygen from the air and form thirty-two grains of sulphurous acid. This is the compound formed by burning sulphur in the air. It is a gas very soluble in water, and possesses strong bleaching powers, as is seen by its use in bleaching straw and woollen goods. Its action upon the organs of respiration is well known by the choking sensation experienced when sulphurous acid gas is breathed in a concentrated state. No doubt the respiration of air containing this acid in close rooms where gas is burnt, is very injurious to the delicate mucous linings of the air passages. Sulphurous acid exposed to moist air rapidly absorbs oxygen and becomes sulphuric acid, which unites with water and

forms the corrosive fluid called oil of vitriol. SO_2 takes up oxygen and becomes SO_3, which unites with water (HO), and becomes SO_3, HO, hydrated sulphuric acid, or oil of vitriol. The bleaching power of sulphurous acid enables the products of combustion from gas to destroy the colours of textile fabrics with which those products come into contact, and the vast damage done by sulphur in gas is matter of general experience and complaint. The sulphurous acid which attaches itself to goods soon passes into sulphuric acid, and this rots the binding of books and other costly articles, as long and varied experience has proved in but too many cases. Advantage is taken of the conversion of sulphur into sulphurous acid by burning to test gas for sulphur. It is burnt slowly so as to oxidise all the sulphur it contains, and the products of combustion are condensed and collected, treated with chlorine or nitric acid to convert the sulphurous acid into sulphuric, which is then precipitated by a salt of baryta, and weighed as sulphate of baryta. Practice soon showed that but a portion of the sulphur could be obtained by merely burning sulphuretted gas and condensing the products, and that to approach at all towards accuracy, it was necessary to unite the acid to a base and form a salt, and thus arrest it. The base employed for this purpose is ammonia, which is volatile, and is carried up into the flame by the current of air which supports combustion. It unites with the sulphurous acid, forms a salt, and condenses in the receivers as sulphite of ammonia. This salt is oxidised by chlorine or by nitric acid, the sulphuric acid is combined with baryta and weighed as sulphate of baryta, and from this the sulphur is calculated.

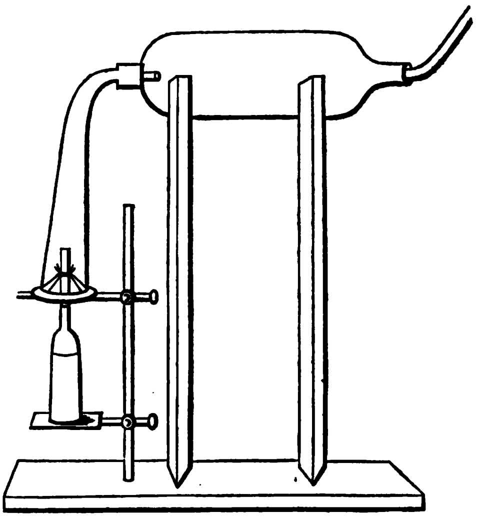


The apparatus generally used for this mode of testing is shown in the margin. It is commonly, but most erroneously, called a test for *bisulphide of carbon.* It does not show the presence of any sulphur compound in particular, but of sulphur compounds in general, without in any way enabling us to ascertain what sulphur compounds are present in and what absent from the gas thus tested.

It is well to insist upon this strongly, as many people, who, from their position and pretensions ought to know better, assert and believe that it is a bisulphide of carbon test. This apparatus consists of a burner, A, placed over a drawer, G; a connecting tube, C, which passes from the flame into the inner tube, F, of a Liebig's condenser, D. To use the apparatus, the drawer, G, is filled with the strongest liquor ammoniæ, a slow stream of water is passed through the condenser by R and S, so as to keep the centre tube, F, cool, and a basin or beaker, E, is placed so as to catch what runs from F, and when all is prepared the gas is lighted at the jet A, having previously passed through a test-meter or a graduated gasholder, whereby it is measured. The water, sulphurous acid, &c., produced by combustion, mix with the ammoniacal gas arising from G, and coming into contact with the cooled tube of the condenser, they run together as fluid from its outlet end into the receiver.

This apparatus is useful to show the presence of sulphur in gas; but it is not trustworthy when the *quantity* of sulphur is to be determined. Part of the sulphur is lost in the process, and the results always come out below the truth. I should have no confidence whatever in any sulphur determinations made by combustion and condensation with this apparatus. I believe this was first made by the late Mr. Wright, of Millbank.

Dr. Letheby has devised one, in my opinion, far superior. Recently, attempts have been made to deprive him of the credit of this apparatus, but I think most unfairly. I saw it first, as used by him, eleven or twelve years ago, and I understood then, and believe now, that the credit of devising it was his. It consists, as will be seen by the annexed sketch, of a Leslie's Argand burner, beneath which is placed a bottle containing the strongest liquor ammoniæ. A tube passes into the bottle and through the burner, so as to deliver the ammoniacal gas just above the jets of flame. Over the burner is placed

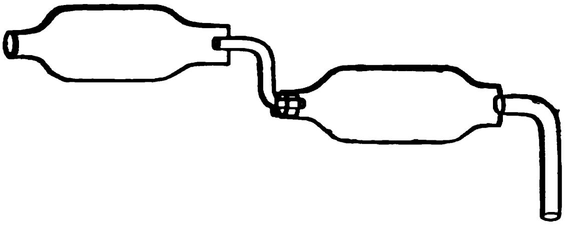


a conical glass tube, which passes through a cork into a receiver made like the cylinders of electrical machines. Out of the other end of this cylindrical receiver another tube passes through a cork, to act as a chimney, and is so bent that anything condensed in it must run back into the receiver. The gas is burnt at the rate of about half a foot an hour, which allows a very perfect oxidation of the sulphur. The sulphite of ammonia condensed is converted into sulphate by treatment with nitric acid, and the sulphur is determined as sulphate of baryta. Dr. Letheby uses but one cylinder, and states that he can condense all the sulphurous acid in this. I have not been so fortunate, and always use two; since, several years ago, I found as much as two grains of sulphur condensed in the second receiver. This had passed through the first receiver, and would have escaped, if it had not been arrested by a second. I know that the experience of others corresponds with my own.

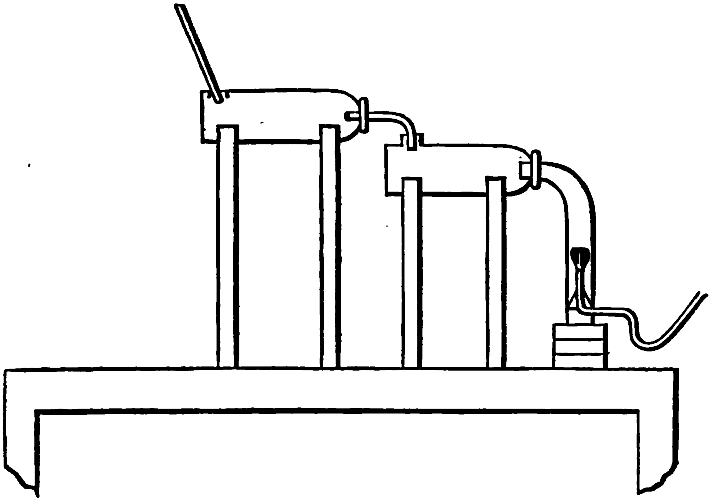
An objection against the use of the apparatus arises from the liability of the gas to be put out by the opening and shutting of doors. This is a valid objection, but it can easily be guarded against by placing the apparatus where no doors are opened or shut during its use, except by the experimenter, who will, of course, take care to move so as not to do harm.

A more serious objection has been urged against it, viz. that it does not really oxidise and condense all the sulphur in the gas, and that the gas always contains more sulphur than is shown by this apparatus. I think it quite likely that there is a small loss of sulphur, and that all estimations made by the use of this apparatus are rather below the truth. This objection does not at all touch the question of whether it is the best practical means we *now* have of ascertaining approximately the amount of sulphur in gas, as I maintain that it is, and as the large experience of sulphur determinations by it in the City of London Laboratory prove. It detects as much as 60 grains of sulphur in 100 ft. of gas of a London gas company, and thus proves itself very useful. In fact, it is too useful to suit the views of certain advocates of existing impurity, who systematically oppose all improvements. If a better apparatus can be produced, we shall all be ready to adopt it; but till a better one is produced, we use this as the best we have. It is a very suspicious circumstance that the objections to this apparatus come from persons in the pay of gas companies, who wish to prove that the quantity of sulphur is so large and so inseparable from gas, that it must be endured, or gas must be given up. Practical people will not fail to see, that when all the sulphur which can be now detected by burning gas in this apparatus has been removed, so that none can be found by burning and condensing the products of combustion, it will be quite soon enough to set about devising something more searching and exact than our present tests.

Mr. Anderson has recently introduced a modification of Dr. Letheby's apparatus (shown at p. 143), but I much doubt whether the single jet will oxidise the sulphur as perfectly as Leslie's burner does, and, consequently, whether the amount of sulphur found will be so high as by the other apparatus. Those who wish to try this modification need not get any fresh apparatus if they possess the ordinary cylinders. They need only make an S tube to join them (as shown at p. 146), and they will answer all the purpose of Mr. Anderson's perforated bottles, and be much easier to clean.

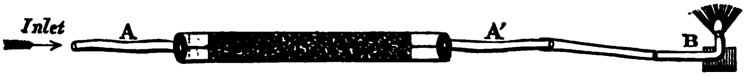


Mr. Anderson's modification of Dr. Letheby's apparatus is shown below, and will be understood at a glance.

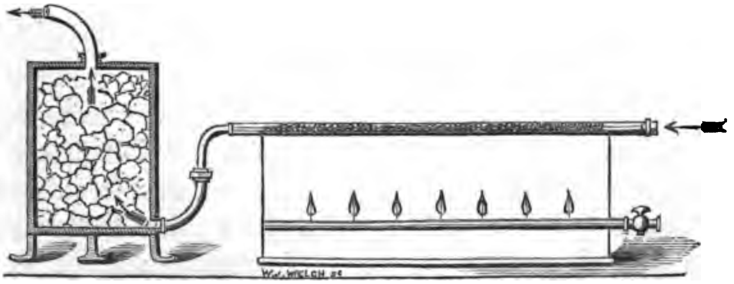


TESTING GAS FOR SULPHUR BY PASSING IT THROUGH HEATED HYDRATE OF LIME.

In 1860, I showed that heated hydrate of lime possessed the power of converting sulphur compounds which are irremovable by the ordinary processes of purification into other compounds which are removable by cold slaked lime and by alkalis. Whether the sulphur be present as bisulphide of carbon, or as sulphuretted hydrocarbons, it is rendered removable by passing gas through the heated hydrate of lime. To test for sulphur by this process I sift slaked lime through wire gauze having forty-nine meshes to the square inch. Dry the coarser portion of the lime which will not pass through the gauze, and fill a glass tube of $\frac{3}{4}$ in. diameter and about 14 in. long with the lime, all but about 2 in. at each end of the tube. The tube is closed at each end with perforated corks, which have pieces of tube passed through them of about $\frac{3}{8}$ diameter and 3 in. in length. The gas in the small furnace is lighted, and the jets are kept down to a blue flame. Air is blown through the lime as it becomes warm, until visible steam ceases to issue from the tube with the air, and when that is the case the tube is ready for use. If not wanted, it may be well corked with good corks, and be kept any length of time. A tube ready for use is shown below:



It is important that the lime fill the tube in diameter, or gas would pass through without coming into contact with it. It is important, also, that it should not fill it in length. It is kept off the ends of the tube by discs of perforated zinc, joined together with copper wire, about 2 in. in length.



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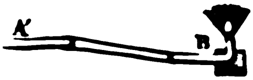

he tube in diameter,
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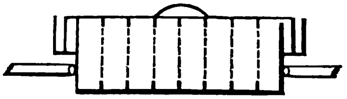
mends the test as very simple and practical."—*Chemical News*, vol. iii., p. 128.

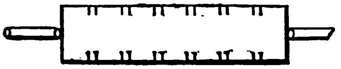
MODE OF CONDENSING HYDROCARBONS FROM GAS.

In the winter of 1853-4 I was anxious to procure a quantity of the condensable hydrocarbons which are contained in coal gas after it leaves the condenser, and before it enters the purifiers, where part are stopped back by the purifying material. To accomplish this, a rectangular tin-plate vessel, about 14 in. long, was constructed and fitted with a series of diaphragms of wire gauze, having 1600 meshes to the square inch. These gauze diaphragms fitted into frames made of tin-plate. The frames were soldered to the top, and slid down grooves soldered to the sides of the box. They were sealed in the bottom by water. The top fitted into a water lute. Having selected a night when it froze hard, the box was exposed to the air, and a stream of crude gas from the purifying-house of a gasworks was passed through it. In the morning, a considerable quantity of condensed hydrocarbons was found floating on the surface of the water in the vessel, which required nothing but separation from the water to be ready for examination. I mention this experiment *apropos* of sulphuretted hydrocarbons, which I know many chemists have failed to condense from gas, whatever amount of cold they have employed. The *only* way, I believe, to condense the hydrocarbons of gas—at all events, a way which never fails, and costs but little trouble—is to pass them through *sufficiently fine apertures* properly cooled. Gas which will deposit nothing in a half-inch tube 4 ft. long, immersed in a mixture of snow and salt, will part with its hydrocarbons readily, if the tube contain diaphragms of wire gauze such as I have mentioned. This gauze is made as an article of commerce to bolt flour, and can be obtained readily. I have not found any form of apparatus preferable to the one I made at first, which I should recommend to be made 18 in. long, 3 in. square, and to contain at least ten diaphragms. Four or five pieces of gauze squeezed together would increase the condensing power.

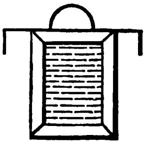
Longitudinal section of apparatus for condensing hydrocarbons:



Plan of apparatus for condensing hydrocarbons, showing grooves in which the frames of the diaphragms slide:



Cross section, showing a gauze diaphragm in its frame soldered to the lid:



It is worth recording here, that the first time I used this apparatus, I separated two cakes of paraffin, each larger than a five-shilling piece, and about the same thickness. The gas from which the paraffin was separated was made from common coal in *iron retorts.* I have since distilled the same coal at a low heat, and have obtained more than 30 gallons of oil from a ton of it. My separation of the paraffin from gas was prior to the trial of Young *v.* the Hydrocarbon Gas Company, and I well remember one of the counsel in that case, beside whom I sat during the trial, wanted to have this fact given in evidence.

Having been the first to separate, examine, and purify the sulphuretted hydrocarbons of gas, I may be permitted to inform those who talk of bisulphide of carbon as if it were the only sulphuretted compound left after ordinary purification, that the most delicate test we have for bisulphide of carbon fails to give any reaction with the condensed fluids and solids obtained from purified gas. Both in my own laboratory and in that of the College of Chemistry they were tested with triethylphosphine without producing the slightest shade of colour. They who know how *a trace* of bisulphide is detected by the phosphorus base, will be assured of its absence in the compounds tested. That these hydrocarbons contained much sulphur was shown by simple distillation, without the trouble of an ultimate analysis; for it was impossible to fractionate them without having sulphuretted hydrogen evolved in abundance. I have frequently been asked what is the form of combination in which this masked sulphur exists in gas. Candour compels me to say that I am unable to give a full answer to the question, but I believe that certain known facts point us to the direction in which a correct answer will be found. I believe the sulphur forms part of a salt of an organic base, such as picoline; for whenever gas which is neutral to test-paper passes over heated hydrate of lime, turmeric-paper is affected as well as lead-paper. Similarly, when coal-tar oils are passed through heated lime, a nitrogenised compound is evolved as well as a sulphuretted one; and when naphthalin is treated similarly the result is similar. I do not remember an instance wherein this has not happened.

The hydrocarbons thus condensed from gas may be tested for sulphur by simple distillation, when they usually evolve sulphuretted hydrogen in abundance, together with other sulphuretted compounds of the most horribly offensive odour. They may be passed through heated lime, or dissolved in ammoniacal alcohol, and so tested, or an ultimate analysis may be made of them, and the *quantity* of sulphur they contain may be determined. If the quantity of sulphur combined with hydrocarbons in gas is to be determined, I recommend the experimentalist to supplement the condensing vessel above described by one containing cumole and cymole which have been perfectly freed from sulphur. These bodies, kept at a low temperature, possess the power of dissolving and retaining a considerable quantity of the light-yielding matter of gas; and, by passing a slow stream of gas through my condenser, and then through a quantity of these heavy oils near the freezing point of water, I should have considerable confidence in securing the *whole* of the sulphuretted hydrocarbons contained in the gas.

Some years after I had separated hydrocarbons from gas by the above apparatus, I met with two curious instances of the power of very small apertures to separate hydrocarbons from gas passed through them. One was in a large two-lift gasholder, wherein there was a very minute hole, through which the gas was forced by the pressure of the holder. The gas could be *heard* to pass through the hole, and could be smelt; but it was in too small a quantity to be lighted. In a fine sunshiny day in spring this small aperture was surrounded by crystals of naphthalin, and yet the gas from which this naphthalin was deposited had an illuminating power of more than twenty candles. Such a thing as a deposit of naphthalin in the mains of that company had never been known, and, prior to my seeing this, I doubt whether the officers of the company suspected that their gas contained such a thing. Subsequently another small leak was found near the station meter, and here again a deposition of naphthalin occurred, though the leak was too small to allow of the gas being lighted.

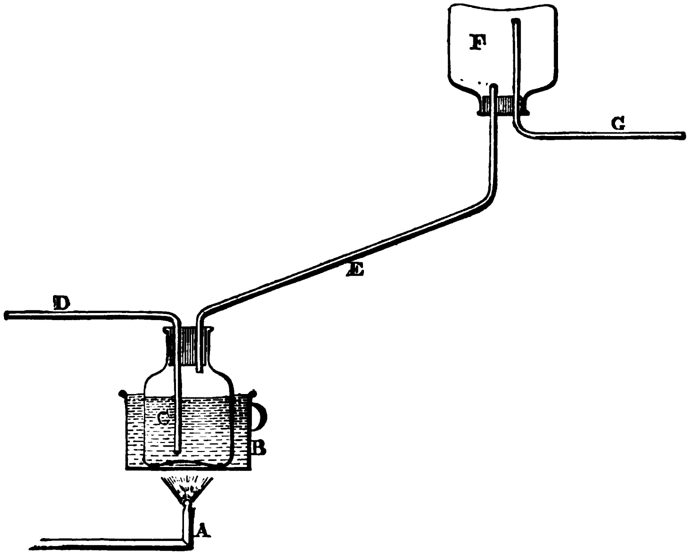
DISSOLVING OUT THE BISULPHIDE OF CARBON BY HOT ALCOHOL, AND PRECIPITATING THE BISULPHIDE BY WATER.

This test was devised by me in 1859, and published in 1860, up to which time the presence of bisulphide of carbon in gas was matter of sagacious inference only; nothing had *proved* that it was to be found in gas. The publication of this test was the first *proof* that chemists had of the existence of this noxious compound in gas. Dr. Hofmann's test by triethylphosphine was not published until 1861. The test is founded upon the facts that bisulphide of carbon is soluble in alcohol, and is precipitated therefrom by water, and, after precipitation, escapes from the diluted alcohol in a peculiar manner. When bisulphide of carbon is dropped into alcohol, it dissolves and forms a clear solution. A copious addition of water dilutes the alcohol and precipitates the bisulphide of carbon as a white cloud. This white cloudy precipitate gradually sinks to the bottom of the vessel which contains it, and leaves the clear dilute alcohol above it. Slowly, bubble by bubble, the white cloudy precipitate escapes through the dilute alcohol into the atmosphere, and, in time (which varies with the quantity present), the whole will pass off, and leave a perfectly clear fluid.

This test is as characteristic as the prismatic ruby crystals obtained with bisulphide of carbon and triethylphosphine, or the brilliant scarlet precipitate obtained with iodide of potassium and protochloride of mercury. It has been objected that the precipitate is *white*, and so of little value. So also are the precipitates obtained by adding hydrochloric acid to a solution of a silver salt, oxalate of ammonia to a solution of a lime salt, and sulphuric acid to a solution of a baryta salt. But it would sound strange to hear these tests objected to on account of the precipitates being colourless. But it is urged we can apply confirmatory tests to these precipitates. So also can we to the precipitate obtained by adding water to a solution of bisulphide of carbon in alcohol. To mention no others, we can add ammonia, and thus form sulphocyanide of ammonium and sulphocarbonate of ammonia, from which we can form a red lime or potash salt, a red-brown lead salt, a chocolate-brown silver salt, and a blood-red iron salt—an amount of confirmation not often possible.

To apply this test to gas, I heat the strongest spirit of wine of commerce to about 160° F., and pass a stream of gas through it for several hours. The flask containing the spirit is connected with a good condensing arrangement. Some of this spirit is diluted with six or seven times its bulk of water. If the gas contain bisulphide of carbon the spirit becomes cloudy upon dilution, and the cloudy precipitate settles down and leaves a clear fluid above. This precipitate passes away slowly in small bubbles, leaving the *whole fluid* clear. This proves the presence of bisulphide of carbon in the gas examined. No other substance contained in gas gives the same reaction.

Apparatus for testing gas for bisulphuret of carbon:



A. Gas-burner or other source of heat.
B. Water-bath. Water kept at 160° to 170° F.
C. Flask containing strong spirit of wine.
D. Gaspipe conveying gas into the spirit of wine in flask.
E. Outlet tube for gas which has passed through the hot spirit.
F. Inverted flask for condensation of any matters which miss condensation in E.
G. Exit-tube for gas which is being tested.

I published this test six years ago, and so far as I am aware, or can learn from those who have given years of patient study to gas, this was the first publication of a test which managers of gasworks can use, and which affords direct evidence of the presence of bisulphuret of carbon in gas. At this moment it is the only practical test known. Mr. Brande was, I believe, the first person who announced the presence of bisulphide of carbon in coal gas, in his Bakerian lecture, published in the "Philosophical Transactions" for 1820.

He says: "The readiness with which carburetted hydrogen is decomposed, when passed through red-hot tubes, appears to me to offer a solid objection to the mode of purifying coal gas, which has been proposed by Mr. G. H. Palmer (*vide* Peckston on Gas Lighting, p. 213), since it would deposit carbon, and consequently sustain great loss in illuminating power. The object in view was probably to get rid of the sulphuretted hydrogen, but neither is this so to be attained. In examining coal gas, I have often been struck with the formation of sulphurous acid during its combustion; though, when passed through solution of acetate of lead, it occasioned no blackening, a circumstance which led me to suspect the presence of some other sulphurous compound: I have often thought, in passing the open gaspipes in the streets, that I perceived the smell of sulphuret of carbon. When sulphurous acid or sulphuretted hydrogen are passed with carburetted hydrogen through a red-hot tube, a portion of carburet of sulphur is always formed, and the vapour of that highly volatile compound may well exist in the gas employed for illumination, which is always hurried through the condensers and gasometer."

Mr. Leigh, chemist to the Manchester Gasworks, obtained a sulphur salt by passing gas through a solution of potash, which salt he thought was formed by bisulphide of carbon. Mr. Herapath infers its presence by the fact of a sulphur compound, arrested by an alcoholic solution of potash from gas which has first been passed through an aqueous solution of potash. Another chemist sought to obtain evidence of its presence, and to retain it, by passing gas over a large quantity of phosphorus, but failed. Mr. Fownes and his able editors say no more than that vapour of bisulphide of carbon is *believed* to exist in gas. Dr. Frankland argues, from the known constituents of coal, that it *must* be present, but affords no evidence that the reasoning is correct. By the simple process which I have published, every manager of a gasworks may detect bisulphide of carbon, if it exist in his gas, without being called upon for expertness in manipulation, or a knowledge of chemistry.

BY DISSOLVING THE BISULPHIDE OF CARBON FROM GAS BY ALCOHOL OR ETHER, AND OBTAINING IT AS A RUBY-COLOURED CRYSTALLINE SALT WITH TRIETHYLPHOSPHINE.

There is a paper by Dr. Hofmann, in the *Quarterly Journal of the Chemical Society*, vol. xiii., p. 85, "On the Detection of Bisulphide of Carbon in Coal Gas." This was published in 1861. I quote so much of the paper as relates to testing for bisulphide of carbon:

"It has long been assumed that the sulphur in purified gas exists in the form of bisulphide of carbon, the conditions for the generation of this compound being, in fact, given in the ordinary process of producing gas. That coal gas really contains bisulphide of carbon was first elegantly proved by Vogel, who, at the suggestion of Baron Liebig, passed a current of purified gas through an alcoholic solution of potassa, when xanthate (sulpho-carbonate) of potassium ($K(C_2H_5)CS_2O$)* was formed, which produced in copper solutions the highly characteristic yellow precipitate of xanthate of copper, and yielded, when boiled with a few drops of nitrate of lead in the presence of free potassa, a black deposit of sulphide of lead. I repeated Vogel's experiments, which I can confirm in every particular. The amount of bisulphide of carbon in the London gas is, however, so small, that a very large volume must be passed through the alcoholic solution of potassa in order to produce a sufficient quantity of xanthate of potassium. After a cubic foot of gas had been passed through alcoholic potassa in a bulb-apparatus, the solution gave with sulphate of copper a leek-green precipitate, in which the presence of xanthate was but imperfectly indicated. Only after passing several additional cubic feet the yellow colour became more distinct, although still masked to some extent by the hydrated protoxide simultaneously precipitated. On the other hand, the black precipitate of sulphide of lead was obtained without difficulty, even after the passage of one single cubic foot of gas.

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* This formula has been objected to. It is Dr. Hofmann's.

"But the presence of bisulphide of carbon in coal gas may be exhibited even more elegantly, and with greater precision, by means of *triethylphosphine*, which produces with the bisulphide a compound crystallising in splendid prisms of a ruby colour. This body is so characteristic, and forms with so much facility, that bisulphide of carbon has become a most valuable reagent for triethylphosphine and its homologues. The idea naturally suggested itself to employ the phosphorus base for the detection of bisulphide of carbon in gas. On distilling a considerable proportion of coal-gas benzole, I had separately collected a small fraction, which came over in the commencement below 65°. When mixed with triethylphosphine, this liquid solidified into a mass of the well-known ruby crystals. Four or five drops of triethylphosphine were dissolved in ether, the ethereal liquid was introduced into a bulb-apparatus, and a current of coal gas allowed to bubble through the solution. When 0·2 of a cubic foot had passed, the liquid had assumed a distinctly red coloration, the intensity of which increased as the passage of the gas and the evaporation of the ether continued. After 0·8 of a cubic foot had passed, the whole of the ether had evaporated, and the inner surface of the bulb-apparatus was lined with a beautiful network of the ruby crystals."

Unfortunately, triethylphosphine cannot be made by any but experienced chemists, and it is not sold (so far as I can learn) as an article of commerce. It is within my own knowledge that five pounds have been offered and refused for an ounce of this compound. It very quickly becomes converted into oxide, when it is worthless as a test for bisulphide of carbon. The beautiful reaction is therefore at present of no avail to practical gas manufacturers. If, however, it can be relied upon to indicate *the quantity of sulphide of carbon*, the subtraction of this quantity of sulphur from the total sulphur found will enable us to approach more nearly than has hitherto been possible to the modes of combination in which the sulphur exists after ordinary purification.

Testing for sulphur by hot copper or brass, query all metals? Observant people must have noticed a white ring upon the Argand burners used in London, and a white cloudy-looking substance upon the upper part of Argand chimneys. The same appearance may be found upon the bell-glasses which are sometimes suspended over gas flames to arrest smoke and keep ceilings clean. This white compound is formed from the sulphur and ammonia in the gas. It is what chemists call sulphate of ammonia. A very excellent way of testing gas for sulphur, when people are not chemists, is to place a piece of brass or copper about three inches above a gas flame where the products of combustion may strike the metal. If the gas contain no sulphur the metal will lose its brightness, and will become superficially oxidised, and slightly smoked; but if the gas contain sulphur, it will become covered at first with a white film which increases gradually, and may at length be swept off as a loose white powder.

For some reason, new metals do not act very well, so that the best plan is to put up a plate, and after it has been in use about a fortnight to brush or wipe off with a dry cloth what has formed upon its surface. The powder of sulphate of ammonia will then form, and may be removed for analysis.

I have mentioned copper or brass, because I have used these metals for years to show sulphur in gas to unscientific persons; but I believe *any metal* will answer. In an iron hood over a sun burner in London I once saw green crystals of sulphate of iron, and a sample of brown fluid which dropped from the iron pipe of a gas-stove in the country I found to contain a double sulphate of iron and ammonia.

CHAPTER VIII.

TESTING GAS FOR AMMONIA.

Ammonia can be detected in gas—

1. By its action upon the colour of turmeric paper, or reddened litmus paper.
2. By its giving a white vapour when fumes of hydrochloric acid are brought into contact with the gas.
3. By passing gas through hot lime and then over test-paper, or through acid.
4. The *quantity* present may be estimated by passing the gas through acid, and determining the quantity which the acid has taken up by test-alkali, or by chloride of platinum.

Ammonia has the property common to all alkalis of acting upon certain vegetable colours, and advantage is taken of this property to detect its presence. It changes the yellow colour of turmeric to a red-brown, and of reddened litmus to a full blue. These changes of colour are temporary only, for, owing to the volatility of ammonia, it flies off and leaves the reddened litmus red, and the turmeric yellow, as they were before they were affected by the ammonia. The fixed alkalis as they are called, potash and soda, do not fly off, but change the colour of the tests permanently.

To apply these tests the paper is slightly damped, so that the ammonia may be absorbed and be brought thoroughly into contact with the vegetable colouring matter, and held within about a quarter of an inch of a gas-jet, such as a fish-tail burner, from which gas is issuing in a full stream. Generally the turmeric paper becomes brown and the red litmus paper blue in less than a quarter of a minute, and thus the presence of ammonia is shown. If the test-paper held thus be not discoloured, it should be damped and put into a glass tube about three-eighths of an inch in diameter connected to a gaspipe by a short india-rubber tube, and a full stream of gas should be allowed to flow through the tube and in contact with the paper for half a minute. If the colour be not affected, the gas certainly does not contain any *free* ammonia. The test is rendered more efficient if a few fragments of potash or slaked lime are interposed in a tube between the gaspipe and the test paper. The alkaline and earthy base are capable of liberating ammonia from forms of combination in which it does not affect vegetable colours.

I must caution inexperienced manipulators against *buying* their test-papers. I have seen purchased turmeric paper held in a full stream of scandalously ammoniacal gas for more than a minute remain perfectly unaffected, whereas properly-made paper became red-brown in a single second. I have known the manager and the entire board of directors of a large gasworks thus deceived. They congratulated themselves upon the purity of their gas tested by some paper of the kind I have mentioned, and were much surprised when shown the real state of the case by proper tests. If any one mean to have useful test-papers, he must make them.

TO MAKE TURMERIC PAPER.

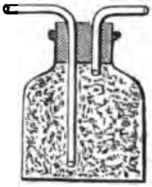
Pour six ounces of spirit of wine upon an ounce of turmeric powder in a clean stoppered bottle. Shake it well once daily for three or four days, and let it stand till the powder has settled to the bottom and left a clear fluid above it. Dip pieces of white filtering paper into this fluid in a clean plate, and when they are thoroughly soaked, dry them upon strings. As soon as dry, cut them into slips, and keep them in a well-corked bottle in a drawer or other place away from light. Turmeric paper must be slightly damped before it is used.

TO MAKE RED LITMUS PAPER.

To an ounce of pulverised litmus add six ounces of cold water. Shake well and allow it to digest. Filter, and add to the solution as much *very dilute* sulphuric acid as will leave the paper distinctly, but not strongly, red when dry. The acid must be added by a single drop at a time. Dip pieces of the best white filtering paper into the solution in a clean plate, and hang them on strings to dry. When dry, cut into narrow strips for use. This paper must be kept in a well-closed bottle in the dark, so that it is protected from both air and light.

Ammonia can be tested for by hydrochloric acid. By dipping a glass rod or a feather into the acid and holding it in a stream of the gas, a white cloud of salammoniac will be produced if ammonia be present in the gas. Its presence may be shown in gas which will not affect test-paper by passing the gas through a tube of hot lime and testing it with turmeric paper after its exit from the tube. I never saw gas which failed to give an ammoniacal reaction when thus treated.

The *quantity* of ammonia present in gas is determined by filling one or two bottles with pebbles and putting into them a known quantity of acid of a known strength. The acid is carefully shaken over the pebbles so that all are moistened with it. The bottles are closed securely with good corks, through each of which two glass tubes pass, one nearly to the bottom of the bottle, and the other just through the cork. The gas to be tested is measured and passed downwards through the pebbles to avoid stoppage of the tubes. It should not be passed at above a foot an hour. At the end of the experiment the acid and ammoniacal salt are thoroughly washed from the pebbles and bottle by distilled water, and the quantity of ammonia is determined by the quantity of acid which it has neutralised, or it is weighed as the double chloride of platinum and ammonium. The latter is the more accurate, but the former is quite accurate enough for a technical process, and is far less troublesome. The methods employed in both cases may be learnt, by those unacquainted with them, from works on chemical analysis.



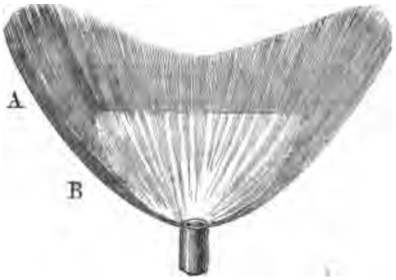
An objection will no doubt be made against what will be called the *severity* of the methods of testing recommended. I anticipate this objection in the future, because it has been made many times in the past. The best answer to such an objection is the fact that gas is sold which will bear to be tested thus. On one occasion, when lecturing in Birmingham, I used about 100 ft. of gas in less than four hours. This gas was passed over lead and turmeric paper of the best quality, so placed in the tubes that the gas must come into contact with them, and during the whole period the colours remained unchanged. It is fair to say that I had held no communication with any officer of the gas company, nor, till the lecture was over, did I know which company's gas was being used; but, upon inquiry, I found it to be supplied by the Old Gas Company. The same care would secure the same result in every town in the kingdom.

BURNING AMMONIA.

In a series of "Miscellaneous Observations" presented to the Chemical Society by Dr. Hofmann, the following occurs:

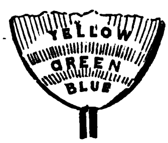
No. 17. "*How to exhibit the inflammability of ammonia.*—It is well known that ammoniacal gas cannot be inflamed in atmospheric air, but will burn in oxygen gas with a greenish-yellow flame. This flame may be shown by allowing the gas to issue from a bent jet into a vessel containing oxygen. There is, however, some difficulty in lighting the gas, and, under the most favourable circumstances, the phenomenon is very ephemeral. To avoid this inconvenience, the inflammability of ammonia is generally exhibited by sending a current of the gas into an ordinary flame, the ammonia gas being allowed to issue from the delivery tube into the lower opening of an Argand gas burner provided with a glass chimney. The gas burning low and almost invisibly, the high lambent ammonia flame becomes very conspicuous. The phenomenon may, however, be observed in a purer and much more brilliant form when a wide-mouthed flask containing a strong aqueous solution of ammonia is heated upon a sand-bath and a rapid current of oxygen from a gasholder forced through the boiling liquid. The mixture of oxygen and ammonia-gas thus formed may be lighted, and burns at the mouth of the flask with the characteristic greenish-yellow flame, which continues until the ammonia is expelled from the liquid."—*Journal of the Chemical Society*, vol. xiii., p. 78.

I am able to add a much more striking mode of exhibiting the ammonia flame with gas than that above



A. Ammonia flame.
B. Gas flame.

described. Gas is passed through sawdust saturated with heavy coal oil, by which it is deprived of much of its illuminating matter, and subsequently through a flask containing a strong solution of ammonia. The mixed coal and ammonia gases are then burnt through a large fish-tail burner (No. 3 of Leoni's answers perfectly), with a roaring flame. A pressure of two inches of water on the gas is required to show the experiment well. The gas flame burns of its usual shape, perfectly white, and *large* enough to give the light of 10 candles, yet it gives the merest glimmer of light. This white gas flame is surrounded with a nebulous ammonia flame nearly as large as a man's hand, possessing the characteristic colour, but less intense than when it is burnt in oxygen. If the gas be now turned down, so as to allow about 3 ft. an hour to pass, the nebulous envelope disappears, and the ammonia burns with the gas between the blue at the bottom of the flame and the

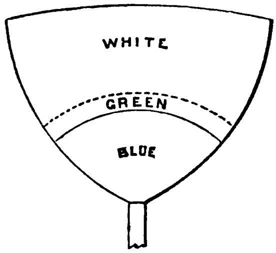


white above, and with a deep grass-green colour. Curiously, this almost entire destruction of light does not occur with Argand burners, but only with flat-flames, fish-tail, and bat-wings, for when the same gas is burnt through an Argand burner, a considerable amount of light is given, and the nebulous ammonia flame is nearly lost. It reappears when the gas is turned low in the Argand, but with less beauty than in the flat-flame. Here is an explanation of one of the well-grounded preferences for Argands over flat-flames, when weak and ill-purified gas is to be burnt. The impurities are far less destructive of light in Argands; but then it shows how little light is obtained for a given consumption of gas in public lamps, which are all supplied with flat-flame burners, and in most private houses which use the same, when gas is of but low illuminating power and withal impure.

An elegant lecture experiment has been published recently, by which the combustibility of ammonia is shown, and the production of nitrous acid as the result of its combustion can be *seen.*

" Take a platinum wire 0·5 mm. thick, and wind it fifteen or twenty times round a lead-pencil, so as to form a spiral; when made, pass one end of the wire through a cork, and let the spiral hang into a wide-necked flask standing on wire gauze over a lamp. The cork must be loosely laid lengthwise over the mouth of the flask. Pour into the flask so much liquor ammoniæ (20 per cent.) as almost to reach to the end of the spiral. Carry a glass tube, about 10 mm. wide, from a gasholder full of oxygen into the flask, so that the end of the tube may dip a little under the ammonia. Now make the platinum spiral red hot, and allow the oxygen to enter. The platinum soon becomes heated to a bright red heat, and the flask is filled first with white vapours of nitrite of ammonia, and then with deep red vapours of nitrous acid; the glass tube which carries the oxygen becomes coated with a thick crust of nitrite of ammonia. If now the lamp under the flask be lighted and the ammonia heated, the mixture of ammoniacal gas and oxygen explodes with a quite harmless explosion. By this the platinum spiral is cooled below the temperature of a red heat; but after a few moments it again becomes a bright red, and the gaseous mixture is exploded as before, so that the experiment goes on repeating itself as long as desired. On introducing a very rapid stream of oxygen, the gas burns for some time under the liquid. It continues to burn, producing the long-drawn sound of the chemical harmonica if the opening of the tube be held immediately above the level of the ammonia and quite close to the platinum spiral. The oxygen-ammonia flame then appears as a greenish-yellow bubble at the mouth of the tube, which may be moved up and down without extinguishing the flame." — *Chemical News*, No. 311. (A lecture experiment by K. Kraut.)

The burning of ammonia in connexion with gas may be shown easily. I showed it to many people upwards of ten years ago. Saturated clay, which has been employed to purify gas, is covered with strong spirit of wine (90 per cent.), and allowed to stand a day or two, when a mahogany-coloured solution is obtained. This is put into an ordinary spirit-lamp and burnt, and the greenish-yellow flame of burning ammonia is shown beautifully. At the bottom of the wick the flame is blue, in the body it is whitish, and at the outer margin it is surrounded with a fine greenish-yellow halo, which remains till about half the spirit is consumed, by which time most of the ammonia is removed from the spirit. The combustion of ammonia may be seen by burning gas which contains much ammonia in an ordinary fish-tail burner. Close to the burner the flame is blue, and above the blue shading into the white portion is a greenish-yellow belt, in which the ammonia is burning. Near the burner hydrogen and carbonic oxide burn with lightless flame. Above, in the white part, the light-giving hydrocarbons burn, and between the two ammonia. This indicates that ammonia is more combustible than the hydrocarbons, but less combustible than hydrogen. The sequence of colour is always that which I have indicated, and it is explicable only on the relative affinities for oxygen of the substances burnt.



TESTING FOR CARBONIC ACID.

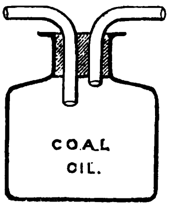
Very little gas sold is free from carbonic acid, and a good deal is said to contain as much as 2 per cent. This cannot happen where gas is purified by lime—except through disgraceful negligence—as lime, in sufficient quantity, will free gas from carbonic acid entirely. It may happen, however, where no lime is employed, and the purification is confined to oxide of iron and acids or metallic salts. Carbonic acid is tested for by passing gas into lime-water, or baryta-water. If carbonic acid be present, a white precipitate of carbonate of lime, or carbonate of baryta, will be thrown down. These precipitates are insoluble in water, and they effervesce and give off carbonic acid when a few drops of hydrochloric acid are dropped upon them.

The quantity of carbonic acid in gas is determined by receiving the gas in a graduated tube over mercury, and passing up a ball of solid potash, or a few drops of a strong solution of potash. The carbonic acid is absorbed by the alkali, and the amount of condensation gives the per-centage of that compound present.

TESTING GAS FOR TAR.

An incredulous smile has been accorded to me more than once when I have spoken of *tar* as a constituent of gas as supplied to consumers. More than once I have answered the incredulous smile by producing tar dissolved out of the gas supplied to my own house, as well as some derived from the gas of other towns, and in answer to the silly question, How I knew it was *tar?* I have produced the hydrocarbon oils yielded by its distillation, and the residue of solid *pitch* left after the oils were distilled off. I have been asked for a definition of tar. I will give one. A thick, brown, stinking, fluid compound of water and sulphuretted and nitrogenised hydrocarbon oils, which furnish ammonia, sulphuretted hydrogen, and other sulphuretted and nitrogenised compounds, together with hydrocarbon oils of various densities, which on distillation leave a residue of shiny hard pitch, which cannot be driven over by heat, but which leaves a porous coke in the retort when the heat has been high enough, and has been long enough applied. This is the well-known gas-tar. It is also the "*tar*" found in, and removable from, gas as it is sold.

An excellent and simple process for obtaining tar from gas is the following: Procure some light coal oil, which is quite colourless, and, if possible, free from sulphur and nitrogen. Place the oil in a wide-mouthed bottle, closed with a good cork, through which are passed two tubes, to carry the gas through the bottle, and so over the oil. Fix this near



the meter, and let the gas supplied to a house pass through it. In its passage the gas sweeps the surface of the liquid, and carries away a portion of it as vapour, depositing in return a part of its tar. As the coal oil diminishes in quantity it becomes darker in colour, and at last there is obtained a residue of the colour of very dark sherry, from which pitch, &c., can be separated by distillation. During the distillation sulphuretted hydrogen, ammonia, &c., will be evolved. I have made this experiment with *absolutely pure* oils, and with a uniform result; so that if an objection should be urged in any case that the coal oil employed *might* have contained some impurity—as, in fact, all commercial samples of oil I have ever seen do—nothing more is necessary than to purify the oils by hot lime, and alkali, and acid, to render objection impossible. I may add to the bare fact of dissolving tar from gas by oils, the very interesting other and little-suspected fact, that some of the oils so dissolved are of such a high boiling point that they cannot be distilled except at a temperature which softens Florence flasks, and that their density is far greater than water. Hitherto I have obtained but small quantities of these very heavy oils, but I hope to obtain a sufficient quantity to enable their properties to be determined with accuracy. They are of higher boiling point than any oils I have obtained from "dead oil," and so possess considerable interest.

Happening to mention these heavy hydrocarbons in gas to Mr. G. Lowe, that gentleman, with his usual candour and courtesy, informed me that this explained what he had often experienced in using his carburetting apparatus. It mattered not how pure the naphtha used in that apparatus was, nor how low its boiling point, there was always left a residue of a mahogany colour, and of that filthy and persistent smell which is characteristic of sulphuretted tarry hydrocarbons found floating on the tanks of gasholders, and which he could not distil. The explanation of this was furnished by my experiments, and the facts proved abundantly the removal of tar from gas by the naphtha employed in carburetting.

Another mode of testing gas for tar is the passing it through clay, and dissolving out the tar from the clay by strong alcohol. If a glass tube, or other vessel containing clay, be interposed between the meter and the gas-pipes of a house, so that the gas must pass through the clay, a few days' use will show several impurities. Strong mineral acids will liberate sulphuretted hydrogen from the clay, alkalies will liberate ammonia, and alcohol will dissolve tar, which, upon distillation, will yield sulphuretted oils, sulphuretted hydrogen, and ammonia, and will leave a residue of pitch.

An alcoholic solution of potash will likewise abstract tar from gas, which will yield its characteristic products upon distillation.

A mineral acid will likewise abstract tar as well as ammonia from gas; and from this it has generally been assumed that the tar was upheld in the gas by the ammonia, and the assumption has been so put as to intimate that the gas would be free from tar if it were freed from ammonia. This is a mistake. The tar is a constituent of the hydrocarbon vapours, and unless these are purified the tar will remain. This is well seen in the case of the hydrocarbons which are condensed from gas, whether they are taken from the surface of the water of the gasholder tank, from the mains, or are obtained by artificial cold applied on purpose to condense them. They may be, and generally are, perfectly neutral, and if not, can be rendered so by washing with water, or very dilute acid; and yet they always contain tar. I have no present recollection of meeting with a sample of hydrocarbons condensed from gas which was otherwise than neutral to turmeric paper, though one or two have affected lead-paper. I have examined numerous samples from London, and from various parts of England and Scotland. The fluid hydrocarbons are uniformly dark brown in colour, from the tar they hold in solution. Upon distillation they leave pitch in the retort, and evolve sulphuretted organic compounds of extreme volatility, sulphuretted hydrogen, and ammonia (perhaps, also, other bases), and yield a quantity of yellowish hydrocarbon oils, which give sulphide of potassium upon the addition to them of a solution of potash. These oils may be purified in the ordinary manner, but they require to be passed through hot lime if they are to be freed from sulphur.

ANALYSIS OF GAS.

Through the kindness of Professor Williamson, I am enabled to publish the new apparatus introduced by him and Dr. Russell for the analysis of gas, together with the important improvements which they made known when the apparatus was published. I give the author's paper entire, and the reader who is acquainted with the gasometry of Bunsen, and with the costly and fragile apparatus of Frankland and Ward—an apparatus possessing much merit, but which it is most difficult to get well made and to keep in order—or with the yet older one of Regnault, upon which Messrs. Frankland and Ward based theirs, will see how great a step in advance has been made by Messrs. Williamson and Russell:

"On a former occasion* we described a simple and accurate process for the measurement of gases, and its application to gas-analysis. The especial advantages of the process are that a smaller number of observations are necessary to ascertain the true volume of a given amount of gas, and that the calculations necessary to deduce, from the observed volume, the bulk it occupies at the standard temperature and pressure, are entirely eliminated. The process thus renders the measurement of a gas more expeditious, and, in making it a simpler operation, renders it more accurate, by diminishing the chances of error.

"The principle on which the process depends is that of using, as a standard of comparison, a constant quantity of air, which is always brought to the same volume. This is effected by marking off, once for all, on the tube containing the air, the height at which the mercury stands; then the effect which any rise or fall of either the barometer or thermometer would produce on the bulk of the air, may be exactly counteracted by raising or lowering the tube in the mercury-trough until the mercury again comes to the mark. The gas in the eudiometer has to be brought to exactly the same tension as that of the standard volume of air. This is done by placing the two tubes side by side, and raising or lowering the eudiometer until the column of mercury within it is of the same height as that which is required to bring the standard quantity of air to the original volume. In the communication before alluded to, we described a convenient form of apparatus for measuring gases by this means, and showed its application to the process of gas-analysis described by Bunsen. Experience having confirmed our views with regard to the advantages of this method of measuring gases, we were naturally led to attempt founding upon it a more rapid method of executing analyses.

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* Proceedings of the Royal Society, vol. ix., p. 218.

"The forms of apparatus invented by Regnault and Frankland have proved to how great an extent the time required for making an analysis may be diminished by surrounding the tube containing the gas with water instead of air, and by using the reagents in a liquid form; nevertheless, neither of those methods, as is well known, has come into general use. This, we believe, arises mainly from the apparatus being of too delicate and fragile a nature. In order to stand ordinary laboratory use, an apparatus of this kind must, we are convinced, be without stop-cocks or other delicate mechanism. It must also be constructed of such a form as not easily to be broken, and these requisites must be obtained without in any way diminishing the accuracy of the process. To construct an apparatus having these qualifications was far from an easy task, and it is only after considerable labour and numerous experiments that we have been able satisfactorily to accomplish this task. We will not lengthen this communication by describing the different forms of apparatus we have from time to time constructed, and the modifications which they have undergone, but proceed at once to the description of the apparatus in its last and most perfect form.

"The tube containing the standard quantity of air we term the pressure-tube. It is represented in Fig. 1. The wide part of the tube is about 7 in. long and $\frac{3}{4}$ in diameter; in fact, it should be a piece of tubing similar to that of which the eudiometer is constructed. The narrow part of the tube is 17 in. long, and $\frac{3}{8}$ in diameter.

"The pressure-tube is carried by a steel rod, 30 in.

FIG.2.

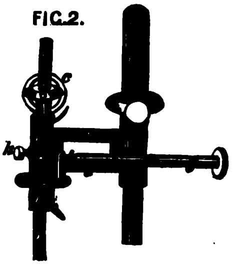


FIG.3.

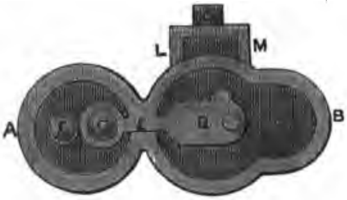


FIG.1.

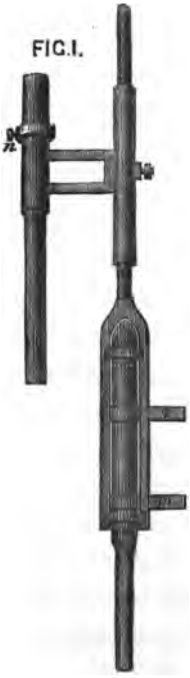


FIG.5.

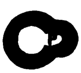


FIG.4.

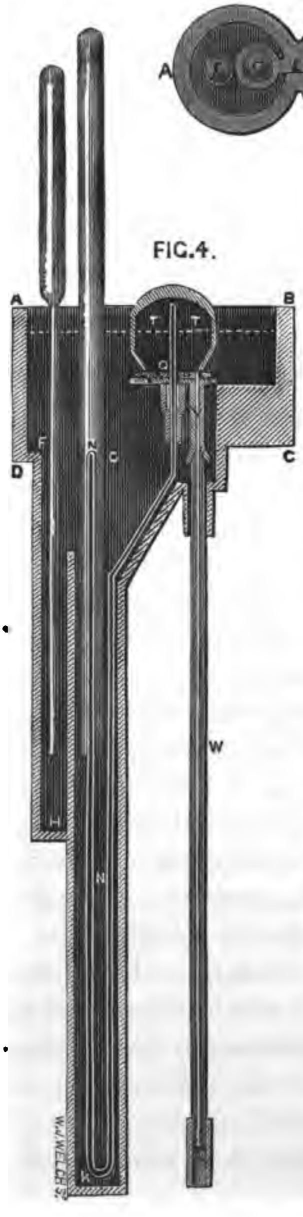


FIG.7.

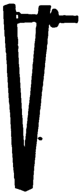


FIG.6.

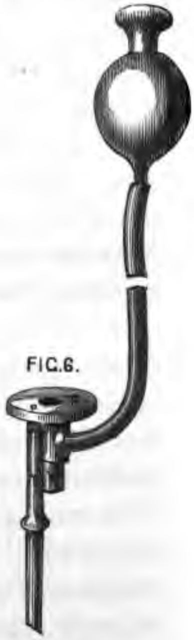
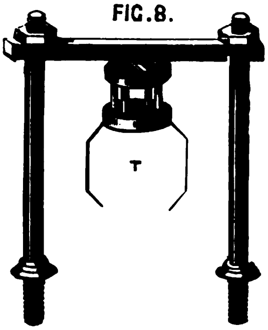


FIG.8.



long and $\frac{3}{8}$ in diameter, at the end of which is a clamp, consisting of two straight bars, $\frac{1}{2}$ in. wide, $\frac{1}{8}$ thick, which extend down the whole length of the wide part of the pressure-tube. Two flexible steel rings are fastened, one near the top of these two bars, the other at the bottom. They are let into and firmly fixed to one bar, and by means of a screw passing through the other bar, the two ends of the ring can be brought together and held securely (Fig. 1). By introducing a little india-rubber padding between these rings and the tube, then screwing the rings tight, the pressure-tube is held with great firmness, and is kept in the same straight line as the rod. Firmly fastened to one of the bars forming this clamp are two other pieces of metal, *l m*, at right angles to it, passing across the back of the tube (Fig. 1). They are $1\frac{7}{8}$ in. long, the upper part $\frac{1}{8}$ thick, but narrowing gradually, so that the lower edge has only the thickness of a very blunt knife. These two cross-pieces serve as marks indicating exactly where the mercury must stand in the tube for the contained air always to have the same volume. We use two marks, in order that when operating with only a small amount of gas it may be considerably expanded, but that when using a large amount of gas, it need not be expanded to the same extent. The height of the mercury in the pressure-tube is altered by raising or lowering the rod in the clamp; but the final adjustment, the raising or lowering of the mercury, so that the meniscus *exactly* touches the lower edge of the cross-piece, is performed very simply and accurately by altering the level of the mercury in the trough. This is done by screwing up or down a piece of large glass tube, which is in the farther end of the trough. We shall allude again to this piece of apparatus when describing the analysis of a gas. It will be seen from what follows that it is absolutely necessary that the steel rod carrying the pressure-tube be perfectly straight, and in order that it may move when raised or lowered in the clamp (Fig. 1), always in one plane, it passes through a tube, which clasps it very accurately, and is 6 in. long; in fact, the tube is made to act like a spring on the rod by having two slits cut in each end of it. The rod, when greased, slips easily through the tube, but is held firmly by it. If, however, the weight of mercury in the pressure-tube should be too great for the spring to hold, the screw, *n*, is used, which, by pressing against the rod, securely holds it at any point. The whole clamp carrying the pressure-tube slides on one of the long, stout rods passing through the table. A groove is cut in the top of the rod for the screw to move in. The object of this is to retain the clamp always in the same vertical plane. To render the pressure-tube ready for use, a very small drop of water is placed at the closed end, and mercury poured in until only a small quantity of air remains. The tube is then inverted in the mercury trough, and more air let in, a few bubbles at a time, until when the pressure-tube is brought to its proper position, the mercury within stands near the upper marker.

"The tube containing the gas to be analysed is an ordinary Bunsen's eudiometer. It is held and moved up and down in the same kind of way as the pressure-tube. The only difference is that the clamp on the eudiometer is very much shorter than the one on the pressure-tube, its length being only 3 in. It is constructed in the same way, only without the two horizontal pieces at the back.

"There is, however, an essential difference in the upper clamp through which the steel rod passes; to this is attached a simple apparatus for raising or lowering the eudiometer through small distances. It is similar to the fine adjustment we described in our former paper on the measurement of gases. Fig. 2 represents this clamp; *a* is the steel rod clamped to the eudiometer; this passes through a stout tube, *b*, and can be firmly attached to it by a screw passing through the top of it, which is thickened for this purpose; the milled head of the screw is seen at *c*. The length of this tube is four inches. Again, this tube is within a second tube, *d*, which is fixed by the bars, *e*, to the tube sliding over the thick rod fastened to the table. The fine adjustment is then effected in this way: a piece of catgut which is fastened to the end of the first tube, *b*, is wound round the axis, *f*, and passes through a hole in the centre of it. The handle, *g*, is attached to this axis, so that when it is turned round, one half of the catgut unwinds, while the other half winds round the axis, and draws the tube either up or down. *h* is a friction screw moving up and down in an opening cut in the exterior tube, and is screwed into the inside tube, *b*. On tightening this screw, it presses more strongly against the outside tube, thus increasing the friction with which the inside tube moves. This arrangement is necessary, as it might often happen when the eudiometer was high out of the trough and nearly full of mercury, that the weight of it was sufficient to unwind the catgut on the axis *f*, and cause the tube *b* to fall to its lowest position.

"The general form of the mercury-trough, which is made of gutta-percha, is seen in Fig. 3, and Fig. 4 is a section of it through A B. The same letters apply to the same parts as seen in both Figs. The sizes given are all inside measurement. The length from A to B is

$9\frac{1}{2}$ in. The thickness of the gutta-percha sides is $\frac{1}{2}$ in., and their height, as seen at B C and A D, Fig. 4, 5 in. The left-hand part of the trough, A E, is $3\frac{3}{8}$ in. in diameter and 5 in. deep. F represents the position of the pressure-tube well, which is attached to the bottom of the trough, and G that of the well for the eudiometer. In Fig. 4, F H and G K represent these wells. The former is 15 in. deep and 1 in. in diameter, the latter is 29 in. deep, and for the first 3 in. after it leaves the trough it is $1\frac{9}{16}$th in. in diameter; the lower part of it has the form shown in Fig. 5, the circular part being $1\frac{1}{16}$th in. diameter, and the projecting part $\frac{1}{4}$ in. wide and $\frac{1}{2}$ in. long.

"In the right-hand portion of the trough, from E to B (Fig. 3), the greater part of it is solid gutta-percha to a height of 2 in. from the bottom, as seen at B C, Fig. 4. The oblong figure in the middle shows the hole which is made there, and the canal joining what may be called the two troughs. L M is a small additional trough of the same height as the large one.

"On referring to the sectional drawing, Fig. 4, the position and arrangement for the movements of the pressure-tube and eudiometer will be easily seen. To the upper part of each of these tubes the clamps before described are screwed, and the steel rods attached pass through and can be held by other clamps fastened to the thick iron rods passing through the table. Into this round part of the trough fits a glass cylinder, about 27 in. high, which is to be filled with water when the apparatus is in use. The cylinder is supported near the top by a ring fastened to one of the iron rods passing through the table, and the mercury in the trough prevents the escape of the water. The cylinder can thus be put on or taken off with great readiness. When the apparatus is in use, the mercury outside the cylinder stands at a height of about 1 in. from the top of the trough. In the method of analysis adopted by Bunsen, the reagents are introduced into the eudiometer. This procedure necessitates the use of only solid reagents, which can absorb the gases but slowly, and besides introducing a small error, namely, that of the gas always adhering to the surface of the reagent, it has the inconvenience, even with the greatest care, of dirtying the inside of the tube, and thus often rendering the reading off of the bulk of gas unsatisfactory. It is quite obvious that, to remove these objections, a separate vessel must be used, into which the gas must be passed when the absorption of any of its constituents has to be effected. This is the laboratory tube of Regnault and Frankland. The form of apparatus best suited for this transference of gas from the one tube to the other, is a point which has engaged much of our attention, and after making many experiments on the subject, we have adopted a process which is exceedingly satisfactory. By the means we are about to describe, the passage of the gas from the one tube to the other is complete, and easily effected; the apparatus is not fragile; and there is no stopcock or other source of leakage. Fig. 4 shows how this transference is effected. Down the centre of the eudiometer-well, a glass-tube, N, of about $\frac{3}{8}$ in. diameter and very small bore, passes; it is bent round at the bottom, curving up in the small addition to the well, P (Fig. 5). When within about 5 in. of the trough, the tube, N, bends, as shown in the figure, but afterwards, again becoming straight, rises vertically into the right-hand part of the trough in the centre, Q (Figs. 3 and 4). Over this end of the tube is passed a vulcanised india-rubber pad with a short tube attached to it. This is shown in Fig. 6, and a section of it at R and Y (Fig. 4). The lower part of the india-rubber tube, Y, is bound with wire tightly on to the glass tube, N, so that the tube projects 3 in. above the top of the circular pad, which is 3 in. in diameter and $\frac{1}{4}$ in. thick. It rests on an iron plate, S, which has a hole in the centre for the tube to pass through. This round pad and iron plate are supported by the solid gutta-percha round the inside of the right-hand portion of the trough. The inside line, Fig. 3, shows how far it extends. The object of the india-rubber pad is to afford an elastic surface for pressing the laboratory tube upon. The laboratory tube, or rather laboratory vessel, we use, is shown in section at T, Fig. 4; it is round, and made of rather thick glass. The height of it is 3 in., so that the syphon-tube, N, reaches exactly to the top of it. The end of the tube should be rounded, so that the middle part is higher than the outside. The laboratory vessel is pressed down on the caoutchouc pad by means of a flat iron bar passing over the top of it. On each side of the trough there is an iron rod fastened into the table; a hole in one end of the flat bar allows it to pass over one of the rods; and a slot in the other end admits of its being brought so far, that the other rod is also in the centre of the flat bar. A screw is turned on the top of the rods, so that by means of a large nut, the bar may be pressed with very considerable force down on the top of the laboratory vessel. Instead, however, of the bar pressing directly on the top of the glass, it is better to have a small round iron frame roughly fitting on it, and having on the upper side a wedge-shaped piece of iron, on the thin edge of which the bar before described presses; by this means the pressure is more equally distributed over the whole of the laboratory vessel. Fig. 8 represents this arrangement.

'This laboratory vessel being thus held firmly, we can first expel the air from it by allowing mercury to flow into it through a tube connected with a reservoir of mercury at the height of about 3½ ft. above it. This tube and reservoir are seen in Fig. 6; the tube forms part of the caoutchouc pad, and enters the wide part of the tube, Y, at right angles. When the pad is in its position in the trough, it passes through a hole in the side, and then through the small supplementary trough L M (Fig. 3), passing out along the gutta-percha tube, U. A short piece of caoutchouc tubing slipped on and bound to both these tubes serves to prevent the escape of the mercury from the trough. The end of this tube is fastened to the vessel, which forms the mercury reservoir. The reservoir is fixed to a wall, or other convenient support, above the trough. The use of the supplementary well, L M (Fig. 3), is to contain a clamp, by which the flow of mercury from the reservoir can be regulated. This clamp is of very simple construction, and is shown in Fig. 7. It is placed in the trough, L M (Fig. 3), before passing the long tube through, so that the tube has to pass between the steel bars. After screwing the nut to a proper position on the rod, we can allow the mercury to flow from the reservoir, simply by raising the rod out of the slot, and allowing the bars to separate, and stop it by again bringing them together and replacing the rod in the slot. By this means we can very conveniently fill the laboratory vessel. The mercury flowing in at the bottom drives the air before it, and causes it to escape through the syphon tube, N (Fig. 4), and the mercury rising gradually to the highest part of the laboratory vessel, expels the last particle of air from it, and then fills the syphon-tube. In this way, the air is expelled at the commencement of an analysis from the laboratory vessel, and a gas, after it has been treated with any reagent, is returned to the eudiometer for measurement. The reverse operation, the drawing the gas out of the eudiometer into the laboratory vessel, is effected by forming a vacuum in this vessel. To accomplish this, there is a small caoutchouc tube, V (Fig. 6), and V (Fig. 4), which is fastened to, or rather forms part of, the pad, R. The diameter of this tube is $\frac{1}{8}$ in. It passes through the pad at a distance of $\frac{3}{8}$ in. from the large central tube. The length of the latter tube is 4 in. It should be made of the best vulcanised india-rubber, and be from $\frac{3}{8}$ to $\frac{1}{2}$ in. in diameter. To the end of the central tube is fastened a piece of thick barometer tubing. The joining is best made by grinding the end of the barometer-tube to a conical form, so that it may be forced up the bore of the caoutchouc tube. Before doing this, melted gutta-percha should be dropped on the end of the glass tube, and after binding the tube on as firmly as possible with iron wire, more gutta-percha may be melted round the end of it, so as to render the joint as perfect as possible. The united length of these two tubes should be about 30 in. The flow of mercury through the tubes is regulated by the following contrivance: at the lower end of the glass tube, a short piece of thick caoutchouc tube is bound firmly on and extends about 1$\frac{1}{2}$ in. beyond the end of the glass tube. A clamp for regulating the flow of mercury from the reservoir, similar in principle to the one before mentioned, passes across this piece of tubing, and, by tightly compressing it, prevents any mercury from passing through. It would, however, to say the least of it, be an exceedingly inconvenient arrangement if it were necessary for the operator to stoop down every time it was required to open or close this clamp. To avoid this, we use a strong pair of crucible tongs for a clamp. The only addition they require is a rod with a screw and nut fastened to one arm, and a slot made in the other, an arrangement precisely similar to that used with the other clamp (Fig. 7). The flexible tube is placed between the jaws of the tongs meant to clasp the crucible, and by opening or closing the other ends, we regulate the flow of the mercury. The tongs must of course be firmly held in one position, so that no strain shall come upon the suction-tube. This is easily done by binding them to a support fixed in the floor. We have now all that is required for effecting the transfer of the gas between the eudiometer and the laboratory vessel. By opening the clamp compressing the tube from the reservoir, and retaining the clamp at the bottom of the suction-tube closed, the gas is forced out of the laboratory vessel into the eudiometer; and by depressing the eudiometer until the top of it touches the end of the syphon-tube, and then opening the clamp at the end of the suction-tube, the gas is forced along the syphon into the laboratory vessel. The end of the syphon-tube, which is within the eudiometer, should be rounded as shown in Fig. 4; every particle of the gas can then be withdrawn from the tube.

"It will readily be seen that this way of filling the eudiometer with mercury is not only simple, but accomplishes the end very satisfactorily; for the air being drawn out from the top of the eudiometer, the mercury rises gradually and regularly, the consequence of which is that no bubbles of air remain adhering to the tube, as they are apt to do when it is filled in the ordinary way. Owing to the elasticity of the syphon-tube, and the upward pressure of the mercury, there is no fear of injuring the eudiometer by pressing it down till the syphon-tube touches the top of it. In order that no bubble of gas may remain adhering to the syphon-tube when the eudiometer is raised, the syphon should be struck once or twice with the end of the eudiometer as it is being lifted up.

"While engaged in working out and perfecting this method of transfer of the gas, we have observed several interesting and important facts, which may be briefly mentioned. First, with regard to the imperfection of the joint made by pressing the laboratory vessel down upon the caoutchouc pad. It will be evident that by means of the screws on the rods, and the large nuts, the laboratory vessel can be pressed down with very considerable force. Besides this, the vessel is surrounded, both inside and out, with mercury to a height of one or two inches, but these precautions are not sufficient to prevent air passing into the laboratory vessel, when a vacuum is formed there. In the experiments made to test whether the vacuum remained perfect, or what amount of leakage took place, the flow of mercury through the syphon-tube was prevented by slipping an india-rubber cap over the end of the syphon, the eudiometer, of course, being removed. By this means it could be ascertained whether any leakage of air occurred after several hours, and even after several days. The mere pressing, however strongly, of the laboratory vessel down upon the pad, and surrounding it with mercury, were insufficient to retain a vacuum; in fact, the air can then enter with considerable rapidity, and is seen to bubble up into the laboratory vessel. The amount of leakage under these circumstances far exceeded what we had anticipated. It evidently arises from the want of contact between the mercury and the glass. A layer of air remains adhering to the glass both inside and out. When it is plunged into the mercury, there is thus a continuous chain of particles of air extending from the vacuum to the level of the mercury outside. The pressing of the vessel on the pad does not produce sufficiently close contact between the glass and the caoutchouc to prevent the gradual movement of this layer of air, so that as the particles at one end of it pass up into the vacuum, others are drawn in from the surrounding air, and pass gradually along. By a very simple proceeding, we are, however, able to prevent any appreciable error from leakage arising in this way; for it is only necessary to moisten the surface of the pad with water, or a dilute solution of corrosive sublimate, before covering it with mercury, and to fill with water the small hollow round the laboratory vessel produced by the expulsion of the mercury by the glass. When these precautions are taken, and other sources of error to which we shall immediately allude are eliminated, we are able to retain in the laboratory vessel an all but perfect vacuum; a vacuum, it is believed, far exceeding that obtained by any other means of this kind. In fact, if a vacuum be retained for twenty-four hours in the laboratory vessel, it will be found that the air which has entered, when reduced to atmospheric pressure, does not occupy a space much larger than that of a large pin's head; and it is probable that this small bubble of air has diffused through the water into the vacuum.

" For some time after this method of passing the gas between the laboratory vessel and eudiometer as first adopted, we used the flexible tube and reservoir, both for increasing and diminishing the pressure on the laboratory vessel. To do this, it was only necessary to raise and lower the reservoir, which was easily done by means of a cord passing over a pulley: this would have rendered unnecessary the second or glass tube connected with the pad, and there would have been no occasion for the transfer of mercury from one vessel to another. We were, however, obliged to abandon this method, after making numerous experiments on the subject, for the simple reason that it was impossible to obtain any flexible tube perfectly air-tight. Tubes of several kinds were made on purpose for these experiments; some were made with a small bore and composed of three or four layers of the best india-rubber, very carefully joined, so as to form one thick tube, which was slightly vulcanised. Mr. Siemens was good enough to make us a tube of non-vulcanised india-rubber, the joinings of which were united by his machine. Painting the tubes over with a solution of india-rubber or gutta-percha in benzol, while a vacuum existed in the laboratory vessel, was tried, but without success. With by far the greater number of the tubes used, the amount of leakage was very small, and when the apparatus is in use, the vacuum, or a near approach to it, instead of being retained for many hours, as was the case in these experiments, only continues for a very few seconds; still, as the small quantities of air thus drawn in might accumulate in the tube, and then rise into the laboratory vessel, we considered it a sufficient reason for abandoning this plan for varying the pressure. Several methods were tried in which an intermediate vessel was used for collecting any air sucked into the tube; but this addition rendered the apparatus more complicated, and did not altogether give satisfactory results. With the glass tube adopted there is perfect freedom from leakage, and it adds but little to the size of the apparatus or to the trouble in manipulating. Fig. 9 represents a general view of the apparatus.

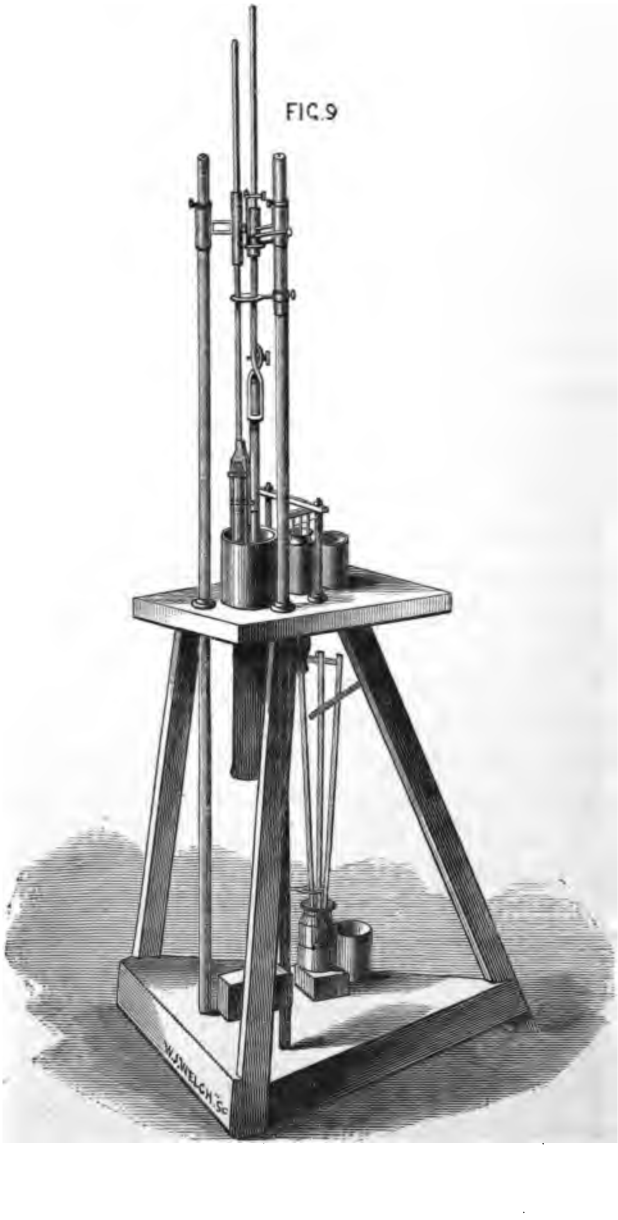


FIG.9

"We now proceed to describe the method of using the apparatus and the precautions to be attended to. When filling the trough with mercury, care should be taken that air does not remain lodged under the caoutchouc pad; also, that the pad and neighbouring parts are well wetted, so as to prevent air being sucked in in the manner before described; in fact, it is best to fill the trough first with water and then pour in the mercury; this process also wets thoroughly the end of the small caoutchouc tube (Fig. 6), into which the glass one is fastened; if this is not attended to, a leakage might take place here in the same manner as at the laboratory vessel. The flexible tube connected with the mercury reservoir should also be well wetted before filling with mercury, and a little water always kept on the mercury in the reservoir; for if this is not done and the tube become dry, air can pass along it, and, in time, may find its way into the laboratory vessel. The suction-tube is easily filled by first filling the laboratory vessel, and then opening the clamp at the bottom of the glass tube, and the clamp on the reservoir tube may also be opened at the same time if necessary to drive the last bubbles of air out. A wide-mouthed bottle, or any other convenient vessel of that kind, may be used to catch the mercury as it flows from the suction-tube, the end of which should always be kept below the surface of mercury. A convenient arrangement is to have a Wurtemburg syphon, one limb of which dips into this bottle, so as to retain the proper amount of mercury in it, and the other limb passes into a second vessel. The mercury, flowing down the suction-tube, runs first into the bottle and then along the syphon into the other vessel. When this mercury is again required, the vessel containing it is removed from below the syphon, and the mercury poured out of it into the upper reservoir. When the trough and tubes are properly filled, the mercury should be dried, the end of the syphon-tube closed by the caoutchouc-cap, and a vacuum formed in the laboratory vessel. This should be left for some hours, then the cap removed from the end of the syphon-tube, and the laboratory vessel filled with mercury. This expels through the syphon-tube all the air or water which has been sucked in. It may be necessary to repeat this process two or three times; in fact, until, after leaving the vacuum for twelve or fourteen hours, it is found that nothing more than an exceedingly small bubble of air has entered. The apparatus should always be put to this very severe test before using it. When not in use it is well to keep the surface of the mercury wet.

"The telescope, or rather magnifying-glass, used in observing the height of the mercury, consists of a single convex glass set in a tube. The opening at which the eye is placed is very small; this avoids any appreciable error from parallax. Within the tube are two fine platinum wires crossing in the centre at right angles. The straight brass rod, about 2 ft. 3 in. long, carrying the telescope, is fixed into a heavy stand provided with three levelling-screws; this rests on the table which supports the mercury trough. The telescope is fastened to a short arm fixed to a brass tube 6 in. long, which clasps the vertical rod, and to prevent this tube having any lateral movement, the ends of it have a slit cut in them, so that they act as a spring, an arrangement similar to that used with the pressure-tube, and before described. A small ring provided with a screw passing through it also slides on the rod, so that it can be fixed to the rod at any point. The tube carrying the telescope rests on this ring, and is free to move in a horizontal plane. We can thus read off the height of the mercury in the eudiometer and pressure-tube. These tubes being close together, the angle through which the telescope has to move is small. The focal length of the magnifier used should be about 6 or 8 in.

"Let us now suppose a gas introduced into the eudiometer, and the cylinder filled with water; the volume of the gas is read off in the following way: The pressure-tube is raised or lowered by means of the clamp and rod attached to it, so as to bring the mercury in it about to the level of the lower edge of the horizontal marker which is to be used; next, the eudiometer is adjusted in the same kind of way, so that the column of mercury in it is about the same height as that in the pressure-tube; the telescope is then adjusted to the proper height, and directed to the mark behind the pressure-tube, the mercury raised or lowered, as the case may be, by giving a screw movement either up or down to the glass tube, dipping into the mercury at the end of the trough. This tube is simply held by a clamp fastened into the table, and moves in it with considerable friction. By this means, while looking through the telescope, the mercury in the pressure-tube may, with the greatest accuracy, be made exactly to touch the bottom of the horizontal mark, which thus forms a tangent to the meniscus. The readings off are rendered much more certain, and are accomplished with much greater ease, if a screen of silver paper is placed at a short distance behind the apparatus, and in front of the source of light, which may be either an Argand burner (the most convenient) or a window. The paper produces a diffused light over the whole apparatus, which renders the different parts, as seen through the telescope, very distinct.

The two markers are made $\frac{3}{8}$ in. deep, so that they throw a shadow on the meniscus of mercury, thus rendering it quite black. The illuminated silver paper forms the background, and gives the greatest possible distinctness to it. A slight touch now moves the telescope in the same horizontal plane, so that the eudiometer comes into view. The two markers, *l* and *m*, Fig. 1, it will be seen, project beyond the pressure-tube, and reach to about half way across the eudiometer, and the next operation is to bring the mercury meniscus in the eudiometer to such a height that it also exactly touches the lower edge of the marker. This raising or lowering of the mercury is effected by means of the fine adjustment before described attached to the upper clamp, which holds the eudiometer. The bringing of the mercury to the required height is as easy as in the former case; the only difference is that the marker, extending in this case only half way across the tube, leaves half of the meniscus illuminated, while the other half behind the marker is quite dark, similar to the meniscus in the pressure-tube. After adjusting the mercury in the eudiometer, so that the marker behind it is a tangent to the highest part of the meniscus, the telescope should again be turned to the pressure-tube, in order to ascertain whether any appreciable alteration of the height of the mercury in it has taken place, by the raising or lowering of the eudiometer; and if so it is readjusted, which is but the work of a moment. The mercury in the eudiometer is again brought to the mark, and the height of the top of the meniscus read off on the graduated scale of the tube. Since the right-hand half of the meniscus has no marker behind it, this is easily done, the diffused light behind rendering the graduation on the eudiometer particularly distinct. If the light behind should be so strong as to render the illuminated half of the meniscus somewhat indistinct, it is brought out with great sharpness by holding between it and the paper screen any opaque body, so as to throw its shadow on the meniscus, but without cutting off the light immediately above the meniscus. It is most convenient if the figures are written on the right-hand side of the scale etched on the tube.

"In a former part of this paper, we have described the clamp which holds the rod carrying the pressure-tube, and have dwelt on the necessity of this rod being quite straight. It will now be evident that it is necessary, for the accuracy of this method of measuring a gas, that the pressure-tube should move up and down, so that the two markers attached to it always remain in a horizontal direction. The clamp sliding on the pillar passing through the table is always screwed to it exactly at the same place as shown by a mark on the rod. Before using the apparatus, it must be ascertained that the two markers are perfectly horizontal, and that they maintain this position when the pressure-tube is raised or lowered to its fullest extent. The best method of doing this is to place immediately behind one of the markers a small glass vessel, with flat sides, containing mercury; then, on raising or lowering the tube, or by altering the amount of mercury in the vessel, the surface of the mercury may be brought close up to the lower edge of the marker, and by comparing the two throughout their whole length, especially when the distance between them is very small, it is at once seen whether the marker is horizontal. The observation may be made with the telescope, the paper screen and lamp being used. If this test shows the markers not to be horizontal, they are brought into that position by altering the level of the table, which should be a three-legged one. This is done either by screws fixed to the legs, or simply by introducing very thin wedges under the legs.

"It will be obvious that there are several advantages in having the lower part of the pressure-tube smaller than the upper part, for it then requires a smaller well to move freely up and down in, and less mercury to fill both tube and well. Care must be taken that the pressure-tube does not at any time touch the sides of this well; for if it did, that would, of course, throw the markers out of their horizontal position. The tube, on being tapped, should always vibrate readily; this shows that it hangs free. Again, by diminishing the amount of mercury in the pressure-tube as much as possible, we diminish the tendency it has to draw the iron pillar which supports it out of the vertical direction. When once properly adjusted, we do not find these markers liable to alter their position. All the readings are made with the gas saturated with moisture. A drop of water is introduced into the eudiometer before filling it with mercury, and enough of it always remains adhering to the tube to saturate the gas, even if the eudiometer has, in the course of an analysis, been filled two or three times with mercury. A very small quantity of water, as before mentioned, is also introduced into the pressure-tube to saturate the standard volume of air. Any alteration in the temperature of the water surrounding the tubes during an analysis will not, as before pointed out, affect the accuracy of the results, provided that both tubes have undergone the same change of temperature. Since they are surrounded by some quantity of water, this condition is easily ensured by thoroughly agitating the water from time to time. This may conveniently be done by means of a wooden stirrer, having the form of a hoe.

"To give an idea of the accuracy of this method, we may cite a few consecutive analyses of air freed from carbonic acid:

1st Specimen of Air.

Air employed	202·83	Oxygen	21·002
After addition of hydrogen	363·84	Nitrogen	78·998
After explosion	236·04		
			100·000

Air employed	213·87	Oxygen	20·997
After addition of hydrogen	376·49	Nitrogen	79·003
After explosion	241·77		
			100·000

Air employed	252·42	Oxygen	20·991
After addition of hydrogen	419·58	Nitrogen	79·009
After explosion	260·62		
			100·000

2nd Specimen of Air.

Air employed	201·18	Air employed	211·10
After addition of hydrogen	353·34	After addition of hydrogen	375·37
After explosion	226·74	After explosion	242·59
Oxygen	20·976	Oxygen	20·966
Nitrogen	79·024	Nitrogen	79·034
	100·000		100·000

Air employed	194·88	Air employed	209·66
After addition of hydrogen	337·38	After addition of hydrogen	375·06
After explosion	214·83	After explosion	243·21
Oxygen	20·961	Oxygen	20·962
Nitrogen	79·039	Nitrogen	79·038
	100·000		100·000

"It will be seen that in these analyses a comparatively small bulk of air was operated on, so that even a small absolute error would have led to a considerable alteration in the per-centage composition of the air.

"It now only remains for us to describe the method of applying the reagents. These are used generally in a plastic form; for instance, if a gas has to be subjected to the action of caustic potash, more or less of the inside of the laboratory vessel is covered with the reagent, by means of a wire brush. To obtain the potash of the proper degree of consistency, it is fused with a little water in a silver crucible. The small brush, made by binding a number of short pieces of thin iron wire together, is from time to time dipped into it, and the potash adhering to it spread on a piece of glass. The evaporation should be carried on till the trial-drop becomes solid on cooling, but still has a translucent appearance. The lamp is then immediately removed from under the crucible, and the potash is painted on the inside of the laboratory vessel round the middle, but not carried far up the conical part of it. Immediately after this operation is completed, the glass is placed in its position over the syphon-tube, screwed down, and filled with mercury, the eudiometer having, of course, been previously raised out of the well and placed on one side, where it is supported by its clamp. The application of the reagent in this way has several advantages over any other method that has been proposed. If any air should adhere to the surface of the potash, the means exist of entirely removing it. This is done simply by forming a vacuum in the laboratory vessel; the air immediately diffuses into it, and is expelled through the syphon-tube by allowing the mercury to flow into it from the reservoir. After the gas has been in contact with the potash, a vacuum is again formed, and the adhering gas, if any, passed into the eudiometer.

"The caustic potash, when painted on in the proper condition, absorbs carbonic acid very rapidly, the absorption of a large amount of carbonic acid being completed in five minutes. The special object of not painting the top of the laboratory vessel with potash is, that in case a little of it should become liquid and float on the mercury, this will attach itself to the glass and not be carried into the syphon-tube. In case, by any accident, potash should be carried into the syphon-tube, it is easy to draw a little water through it, which will, of course, immediately remove it.

"The following are two analyses made of a mixture of air and carbonic acid:

Volume of gas taken	248·02	Volume of gas taken	230·63
After absorption by potash	201·80	After absorption by potash	187·56
After addition of hydrogen	356·80	After addition of hydrogen	331·11
After explosion	231·25	After explosion	214·17

Carbonic acid	18·64	Carbonic acid	18·68
Oxygen	16·87	Oxygen	16·90
Nitrogen	64·49	Nitrogen	64·42
	100·00		100·00

"For the absorption of oxygen, the simplest way of applying pyrogallate of potash is to prepare the potash in the way before described, and then just before painting it on the laboratory vessel, some pyrogallic acid is added, and the mixture applied by means of the wire brush.

"The fuming sulphuric acid for the absorption of hydrocarbons is best applied by means of a coke ball.

The gas is passed over into the laboratory vessel, the bar which presses it down taken off, and the coke ball introduced in the ordinary way; the absorption of the hydrocarbons takes place very rapidly. A potash ball is afterwards introduced to absorb the acid vapours. As far as the form of this apparatus is concerned, it will be evident there is nothing to prevent the use of solid reagents in all cases, if thought desirable.

"The following are analyses of mixtures of known quantities of different gases:

Volume of air employed	182·45
After addition of hydrogen	213·84
After addition of carbonic acid	257·98
After absorption by potash	213·84
After explosion	166·72

	Employed.	Found.
Carbonic acid	17·11	17·11
Hydrogen	12·17	12·17
Air	70·72	70·72
	100·00	100·00

Volume of nitrogen employed	95·76
After addition of carbonic acid	112·14
After addition of oxygen	132·73
After absorption by potash	116·27
After addition of hydrogen	187·70
After explosion	125·88

	Employed.	Found.
Carbonic acid	12·34	12·40
Oxygen	15·51	15·52
Nitrogen	72·15	72·08
	100·00	100·00

"In the last analysis, only a very small quantity of gas was employed, and all the readings, except the one after adding hydrogen, were made by using the lower mark on the pressure-tube, and thus expanding the gas to about double its volume at atmospheric pressure.

"Oxygen or hydrogen is introduced into the eudiometer in the same way as in the ordinary process; it is only necessary to use a longer delivery tube, the end of which is introduced within the glass cylinder, by dipping it down the narrow canal joining the two circular troughs.

"The best way of introducing into the apparatus the gas to be analysed, will depend upon the form of the vessel which contains it; if it should have a delivery tube, the gas can at once be bubbled up into the laboratory vessel, which has been previously filled with mercury; if it is contained in a tube, this is passed down over the syphon-tube and the gas sucked out of it into the laboratory vessel. The tube containing the gas is easily brought over the syphon, either by removing the large glass cylinder, or by fastening a stick to the tube, which is done with a couple of caoutchouc rings, dipping the open end of it in a cup of mercury attached to a long rod, and thus lowering it into the mercury-trough. A third way is to bubble the gas at once into the eudiometer and then transfer it to the apparatus."*

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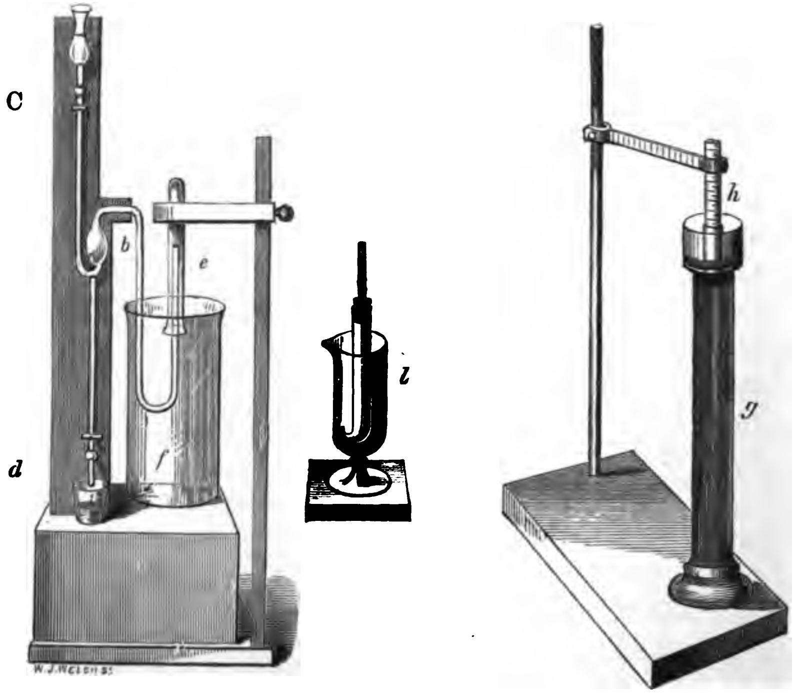
* The complete apparatus may be obtained from Mr. Harrison, 68, Red Lion-street, Clerkenwell-green, London, E.C.

MILLER'S TRANSFERRING PIPETTE FOR GASES.

"The analysis of these gases, or of any mixture of air with carbonic acid, such, for example, as respired air, may be effected with sufficient accuracy for most purposes in the following manner: Supposing that the gas had been collected over either water or mercury, it becomes necessary to transfer a portion of it from the jar in which it has been collected to the one in which it is to be analysed. A method of effecting this is shown below. Upon the board A *d* is fastened a pipette, designed for effecting this transfer. A is a cylindrical funnel, of a capacity of about 2 cubic inches; at C is a small steel stopcock, or a piece of vulcanised caoutchouc



tubing, compressed by a screw, which is simpler and less expensive. By either of these contrivances the contents of the funnel can be admitted to a wide thermometer tube, which is furnished at *d* with a second steel stopcock, or caoutchouc tube, and screw clamp; *b* is a bulb of the capacity of one cubic inch, or rather less; from the upper part of *b* proceeds another piece of thermometer tube, bent as shown in the figure, to allow of its introduction into the gas-jar. To use the instrument, the funnel A is filled with mercury, the stopcocks are both opened, and as soon as the air has been displaced from the vertical portion of the fine tube, and mercury escapes through *d*, the stopcock *d* is closed; the mercury quickly displaces the air from the rest of the tube, and from the bulb *b*, and as soon as it begins to flow out at the open extremity of the recurved portion, the stopcock C is closed. The instrument being now full of mercury, is introduced into the jar *e* of the gas to be transferred, and its open extremity is raised above the level of the water in the jar *e*; the stopcock *d* is then opened, and whilst the mercury runs out into a vessel placed for its reception, the gas enters from *e*, and occupies the place of the mercury in the bulb *b*. When a sufficient quantity has been admitted, the tube is depressed below the level of the water in the jar *e*, the stopcock *d* is closed, and the pipette, which is sealed by the admission of a little water into the capillary tube, is withdrawn from the jar *f*. The gas can now be transferred to the graduated tube *h*, standing in the jar of mercury, *g*; the bent limb of the pipette is introduced into the tube *h*, which has been previously filled with mercury. Fresh mercury is poured into the funnel *a* of the pipette, and on opening the stopcock, *c*, the gas is expelled into the tube *h*; the gas should not occupy more than two-thirds of the capacity of this tube.

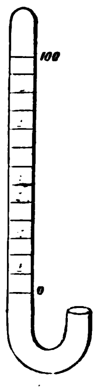
"The proportions of carbonic acid, of nitrogen, and of oxygen, are now easily ascertained in the following manner: The bulk of the gas in the tube *h* is to be carefully read off, care being taken to bring the mercury to the same level within and without the tube, the temperature and the pressure being observed as usual. Supposing that it has been thus ascertained that a bulk of gas of about two-thirds of a cubic inch is to be subjected to the analysis, the operator, by means of a glass syringe, throws up ten or twelve drops of a solution of caustic potash (sp. gr. 1·4) into the tube. The glass syringe may be extemporaneously prepared from a strong tube, which is softened in the flame of a lamp, drawn off and recurred at one end. This constitutes the body of the syringe, whilst the piston is easily formed of a piece of glass rod, provided with a plug of caoutchouc. The operator then agitates the contents of the tube by rapidly thrusting down the tube into the mercury, and withdrawing it, taking care to keep the mouth below the surface of the mercury. This manœuvre is several times repeated in quick succession; the tube is again left at rest for a minute or two, and the absorption is noted by a second time reading off the volume of the gas at the proper level. The difference indicates the amount of carbonic acid.

"In order to ascertain the proportion of oxygen in the remainder, the plan recommended by Liebig is the simplest. A solution of one part of pyrogallic acid in six of water is prepared, and about forty drops of the solution is, by a *fresh* syringe, injected into the tube *h*, and the mixture is briskly agitated, as before; the solution of potash, if oxygen be present, becomes of an intense bistre colour, and the oxygen is quickly and completely absorbed; the gas is measured a third time, and the residue is estimated as nitrogen; the difference between the second and third readings giving the volume of oxygen."

CHAPTER IX.

CONDENSATION BY BROMINE.

Chemists have long known that chlorine possessed the power of condensing olefiant gas, and forming what is generally called Dutch liquid, from its having been first obtained and described by Dutch chemists. Long ago, Henry proposed to condense the light-giving constituents of coal gas by chlorine, and thus to estimate them by the volume which suffered condensation. Chlorine was found troublesome, and bromine was used in its stead with considerable advantage, for it condenses illuminating constituents without attacking the marsh gas when the mixture is exposed to daylight, whereas it was necessary to use chlorine only in the dark. Any exposure of the mixture of chlorine and gas to daylight caused condensation of the marsh gas, and thereby vitiated the experiment. Condensation by bromine is effected by filling a graduated tube with water, and allowing the gas to flow into it, and displace the water until the water-line is at zero. A drop of bromine solution is put into the bend of the tube with a small pipette, the hole closed with the thumb, and the tube shaken so as to bring the bromine solution thoroughly into contact with the gas. Certain hydrocarbons are thereby condensed, and the gas occupies a proportionally smaller space. The bend of the tube is then put into water, so that the hole may be securely covered, and the thumb is removed. A volume of water equal in bulk to the condensation enters the tube. Some potash is put into the tube to absorb any bromine vapour which may remain in the gas, and when nothing but gas is left, and water has taken the place of the potash solution, the amount of condensation is read off. Any dilatation of the gas, by handling the tube, &c., would introduce serious error, so it is customary to place the tube in a cylinder of water before and after the experiment, which secures the same temperature at each reading.



PROPER METHOD OF FILLING THE BROMINE TUBE WITH GAS WHEN THE CONDENSABLE COMPOUNDS IN GAS ARE TO BE ACTED UPON BY BROMINE.

When the amount of hydrocarbons in gas, condensable by bromine, is to be ascertained, the graduated tube in which the operation is performed is usually filled with ordinary water, and the gas is allowed to bubble up through the water until the tube is full. This involves two avoidable errors. First, the water used contains air, which is wholly or to a great extent displaced by the gas. The displaced air mingles with the gas, and being incondensable, augments the residue left after condensation. This makes the condensation appear less than it really is. Secondly, the water dissolves hydrocarbons from the gas, and thus the condensation is reduced still further below the truth.

Another method of filling the tube is by displacement of the contained air by gas passing into it through a small india-rubber tube. This process, again, contains avoidable error. In the first place, great care is required to ensure the perfect displacement of the air, and to be sure that none is admitted when the india-rubber tube is withdrawn; and, secondly, the india-rubber of the tube absorbs some of the hydrocarbon vapours of the gas, which it is the object of the process to condense and measure.

The best method of operating is to saturate some water with the gas to be examined, and then to pass a small india-rubber tube to the bottom of the inverted tube, and to pour the saturated water through this into the tube. If a quantity of water equal to three or four times the volume of the tube be poured through it, the air will be nearly all removed. The gas is then to be bubbled up through the saturated water in the usual way. After condensation, the open limb of the tube should be filled with saturated water, and corked or closed with a stopper before it is placed in the equalising cylinder to attain the normal temperature. Whether this process shows the illuminating value of coal gas can be known by experiment, and by experiment only. If a number of different gases of nearly a similar illuminating value by the photometer give a condensation by bromine, which corresponds to the photometric results, the process is trustworthy; otherwise it is worthless. I am quite aware that this assumes the accuracy of the results obtained by the photometer, and I am quite prepared to assume the responsibility of saying that the results obtained by honest and competent photometrists are a true practical valuation of the gases they examine. They are not scientifically true, neither is one analysis in a thousand, whether mineral or organic, and yet the whole of our chemistry is based upon such analyses.

To test the value of the process, I have collected and tabulated a number of published results, which are sufficiently numerous and various to enable unprejudiced people to form a sound judgment. They include condensations as far apart as 3·75 per cent. and 30·0 per cent., and illuminating powers as different as 37·75 candles and 11·50 for a consumption of five feet an hour.

Names of coals from which gas was made.	1 Cubic feet of gas per ton.	2 Specific gravity.	3 Weight of gas per ton. lbs.	4 Illuminating power.	5 Condensation by Bromine.	6 Factors required to multiply the condensation volume, and give thereby the illuminating power of the gas examined.
Boghead cannel	15,000	·752	866	37·75	30·0	1·26
Lesmahago, 1...............	13,500	·642	666	27·10	16·0	1·69
Armiston......................	12,600	·626	606	22·50	17·0	1·32
Lesmahago, 2...............	13,200	·618	627	24·80	17·0	1·46
Wemyss	14,300	·580	637	24·50	14·5	1·75
Capeldræ......................	14,400	·577	638	19·75	16·5	1·19
Kirkness...............	12,800	·562	552	21·20	10·2	2·08
Knightswood	13,200	·550	558	19·00	9·5	2·00
Ramsey	10,300	·548	433	21·40	12·5	1·71
Wigan (Ince Hall).........	11,400	·528	461	20·00	11·5	1·74
Leverson cannel............	11,600	·523	466	18·00	10·0	1·80
Pelton ditto	11,500	·520	459	18·50	10·5	1·76
Washington ditto	10,500	·500	403	18·00	10·5	1·71
Blenkinsopp	9,700	·450	335	14·00	6·0	2·33
Pelton main	11,000	·430	363	14·00	4·5	3·11
Dean's Primrose............	10,500	·430	347	13·50	5·0	2·70
Washington	10,000	·430	330	14·00	5·0	2·80
Leverson	10,800	·425	353	12·50	4·0	3·12
Hastings' Hartley	10,300	·421	333	12·50	4·3	2·91
Pelaw	11,000	·420	355	12·75	4·5	2·83
West Hartley...............	10,500	·420	339	12·50	4·2	2·98
New Pelton	10,500	·415	335	12·00	4·75	2·53
Gosforth	10,000	·402	308	12·00	4·00	3·00
Garesfield	10,500	·398	321	11·50	3·075	3·07

One way in which the value of this method of estimating the light-giving power of gas may be shown, is by dividing the gases into groups of about the same illuminating power, and contrasting the condensations by bromine, which ought to be nearly the same for the members of each group, if the process be reliable.

Twelve candle group.

	Illuminating power.	Condensation by Bromine.
Gasforth	12·0	4·0
New Pelton	12·0	4·75
Leverson	12·5	4·0
West Hartley	12·5	4·2
Hastings' Blenkinsopp	12·5	4·3
Pelaw	12·75	4·5
Fourteen candle group.		
Pelton	14·0	4·5
Washington	14·0	5·0
Blenkinsopp	14·0	6·0
Eighteen candle group.		
Leverson cannel	18·0	10·0
Washington ditto	18·0	10·5
Pelton ditto	18·5	10·5
Nineteen candle group.		
Knightswood	19·	9·5
Capeldræ	19·75	16·5
Twenty-four candle group.		
Wemyss	24·50	14·0
Lesmahago, 2	24·80	17·0

Those who rely upon condensation by chlorine or bromine to give them the illuminating value of coal gas, multiply the condensation volume by some constant factor which they suppose will bring out the illuminating power in standard candles. If this factor be anything more than a mere fancy number, varied according to each person's caprice, it must be obtained in the first instance from experiment, and since a multiplication of the condensation volume by the factor gives the illuminating power in candles, a division of the illuminating power by the condensation volume must give the factor. Column 6 in the annexed Table shows the results in twenty-four cases. The illuminating power is divided by the condensation per cent., and the figures in column 6 show the quotient; the factor which must be employed in each case to multiply the condensation volume, and produce, by such multiplication, the number of standard candles to which the gas is equal. It is difficult to imagine anything more hopeless than the task of obtaining a constant out of that mass of differing numbers. All notion of *averages* is out of the question, as may be shown easily. The mean of all the quotients is 1·65, a number which differs from every number in the Table. Using this in the case of gas from Armiston cannel, we find the illuminating power to be 28·05 candles instead of 22·50, and in the case of gas from Kirkness cannel, it is 16·83 instead of 21·20. Errors like these at once exclude the notion of average. The more the question is considered, the more evident will become the conclusion, that at present condensation by bromine is entirely useless as a means of comparing the illuminating values of different gases. Whether it is of use to determine the value of gas made day after day by the same process, from the *same coal*, must be decided by careful experiments.

Attempts have been made to show that if the amount of condensation be multiplied by the specific gravity of the matter condensed, the product will represent the illuminating value of the gas in standard candles, as found by the photometer. This is stated by Mr. Lewis

Thompson, and Mr. Banister has tabulated some results of Mr. Evans to show it. I have extracted the material part of his Table, and have added to it a column, which shows by inspection the discrepancy between the photometric results and those obtained by multiplying together the condensation volume and the specific gravity of what is condensed. Numbers having the sign + show that the latter figures are so much in excess of the photometric figures, or that the illuminating power is put so much too high, and those with the sign - that they are in defect, or that the illuminating power is put so much too low:

Name of cannel.	Condensation by Bromine.	Specific gravity of condensed matter.	Photogenic power, candles.		Excess + and defect —
			Actual.	By analysis.	
Boghead	30·0	1·25	37·75	36·30	− 1·45
Lesmahago, 1	16·0	1·64	27·1	26·24	− 0·86
Lesmahago, 2	17·0	1·43	24·8	24·31	− 0·49
Capeldræ	16·5	1·29	19·75	21·28	+ 2·53
Armiston	17·0	1·40	22·50	23·80	+ 1·30
Ramsay	12·5	1·82	21·40	22·95	+ 1·55
Wemyss	14·5	1·87	24·50	27·11	+ 2·61
Kirkness	10·2	1·95	21·20	19·88	− 1·32
Knightswood	9·5	2·28	19·00	21·66	+ 2·66
Ince Hall	11·5	1·77	20·00	20·35	+ 0·35

Even if the results were correct, the labour required by this process must exclude it from practical use, besides its being certain to deceive, except in the hands of experienced manipulators. If the condensation number multiplied *by a constant* would give the value of the gas, it would be a ready process. At present, no such constant is known. All that have been used are fallacious.

To show the worthlessness of condensation by bromine as a measure of illuminating value, Dr. Letheby has published the results of fifty cases in which gas was examined by condensation and by the photometer. One class of cases had a condensation equal to 3 per cent., and the other a condensation equal to 4 per cent. The gas of Class I., with a 3 per cent. condensation, varied in illuminating power from 14·7 candles as a maximum to 10·87 as a minimum when examined by the photometer, so that there was a difference of 3·83 candles with the same condensation. The maximum illuminating power gives 4·90 candles as the value of 1 per cent. condensation, whereas the minimum gives 3·62.

The gas of Class II. varied in illuminating power, by the photometer, from 15·72 to 11·40 candles, giving a difference of 4·32 candles with the same condensation. The maximum in this class gives 3·93 candles as the value of 1 per cent. of condensation, and the minimum gives but 2·83.

These experiments are open to the remark that the photometric indications are too high, owing to the improper burner employed to consume the gas; but, making all allowance for that, in no way affects the conclusion that condensation by bromine is no indication of the illuminating value of coal gas.

After what has been said, it may appear superfluous to add anything more on this subject. I cannot, however, refrain from observing that there are other objections which must be removed before the process can be accepted by chemists as trustworthy. When the gas enters the tube and displaces the water, it passes through the water, which materially alters the composition of the gas, by dissolving out of it a quantity of the luminiferous constituents which it is the object of the process to estimate. Several years ago I showed the damaging influence of water upon gas, and since then Mr. Anderson has reiterated the statement, and added the results of his experiments. Now it is grossly unfair to employ a process which first robs the gas of a quantity of its illuminating constituents, and then professes to measure them. This might be obviated in a great measure, as I have said, by saturating the water put into the tube with gas before it was used, but this precaution has not been observed, to my knowledge.

There is another objection quite as formidable. Those who recommend and employ the process assume that, when condensation has taken place, all the substances formed by the action of bromine on the constituents of the gas (and they are numerous) are liquids, which can be and are poured out, or gaseous compounds, which are removed by water and potash. This is a mistake, and the volume of the residual gas which remains after condensation is increased by the vapour of one or more compounds formed by the action of the bromine upon the constituents of the gas; so that the true amount of condensable hydrocarbons is really greater than appears after the action of the potash.

The following extract adds another to the defects of the process:

"*On the Properties of Acetylene.*"[*] *By M. Berthelot.*

"Considerable differences have been observed between the properties of acetylene prepared by various reactions. Thus, according to M. Rehoul, acetylene prepared by means of bromide of ethylene furnishes tetrabromide, $C_4\ H_2\ Br_4$; while that which I obtained by the decomposition of ether vapour formed a bibromide, $C_4\ H_2\ Br_2$.

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[*] Bulletin de la Société Chimique, February, 1866, p. 97.

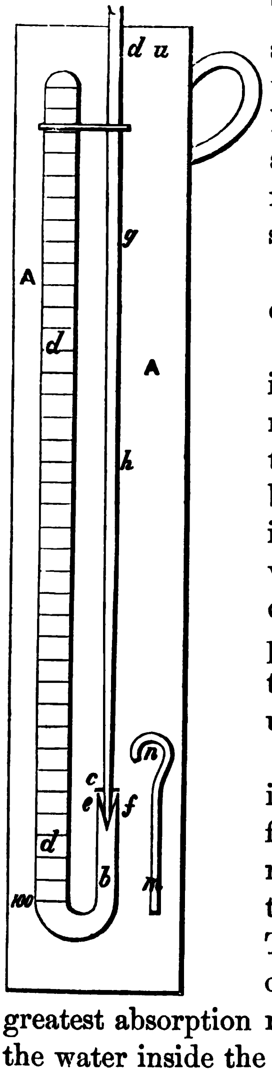
"*According to one observer, whose name has escaped me, the acetylene contained in coal gas is not absorbed by bromine.* Hence the opinion that there existed various isomeric carbides of the formula $C_4\ H_2$, capable of precipitating ammoniacal cuprous chloride red.

"*The facts above cited are correct,* but they do not show the isomerism of the carbides experimented upon."—*Chemical News,* vol. xiii., p. 134.

When lecturing at Birmingham, last autumn, to the Society of Gas Engineers, Dr. Letheby told his audience that acetylene "combines with great energy with chlorine, bromine, and sulphuric acid." The inference which gas engineers would draw from this would be, that when coal gas containing acetylene was treated with bromine, the acetylene would be condensed by the bromine, and so would be removed from the gas. The above quotation from M. Berthelot shows how erroneous the inference is, and how important it is to guard statements, so as to prevent their producing such erroneous impressions.

We thus see that the process is essentially erroneous; but, as we are ignorant of the amount of error, we cannot apply a correction for it, as is done in cases where an error of constant and known amount attends a process.

Mr. Anderson has recently described a form of apparatus for condensing the hydrocarbons in coal gas by bromine, by the use of which he states that "it is not difficult to obtain results of complete accuracy."



The tube, as shown in the annexed sketch, may be more convenient in use than the old form of tube, but how it can in any measure affect the accuracy of a process which is inherently erroneous, I am unable to see. Nevertheless, I give the sketch of the apparatus and the author's description.

"I shall have frequent occasion, in describing the experiments I have made upon this sulphur question, to refer to the employment of the bromine operation, and as I have improved the means of manipulating with this apparatus so that it is not difficult to obtain results of complete accuracy, I shall now proceed to describe how I proceed when I use this method of examination.

"The tube should not exceed 23 inches in length, or it is too clumsy for ready use. Its inside diameter-measurement should be not more than five-eighths of an inch at *d d*. The short upturned limb *b* must be of such a length, that when the greatest absorption required by the operator has raised the water inside the tube *a* to the maximum, the corresponding level can be restored, when necessary, by pouring water into the orifice *c*, in the short limb *b*. The diameter of the tube at *b* should be about, or somewhat less than, half an inch, and the upper part must be nicely lipped and rounded at *e*, so that it will be closed air-tight by a good cork, or the perforated cork with tube fitted into it—*f*, *g*, *h*. With gases whose absorption by bromine does not exceed 12 per cent., if the short limb *b* is constructed 3¾ inches in length, it will be sufficient. When of that length, it answers well for the examination of ordinary coal gas of 3 or 4 per cent. condensation; for if the column of fluid in *b* at any time exceeds in height the water-level of the tube *a*, it can be lifted out to the exact required level by the use of the piece of capillary glass tube *m*, which is joined to a short piece of india-rubber tube, *n*. If the part of this tube *m* be immersed in a column of water in *b*, it becomes filled to the same level; and by pinching the flexible tube *n*, when it is withdrawn, the water it contains is kept within it by the pressure of the atmosphere. Thus the exact necessary level may be adjusted as required; and by the employment of the long tube *g h*, when the levels of the fluids both in *a* and *b* are adjusted, the several readings, which it is absolutely necessary should be made at the exact temperature of the water in which the tube is from time to time sunk, contained in the vessel A A, may be promptly made, without the necessity of taking out upon every occasion the water, which would else flow, when the tube was sunk in the vessel A A, into the short limb *b*.

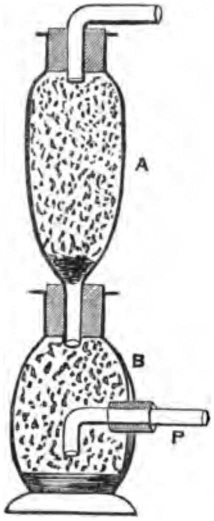
"In reading off, I raise the tube completely out of the little water-tank A A, by attaching a piece of string to the upper part of the tube, running over a pulley with a counterbalance weight, so as to raise the tube to the required height and support it there. The tube, in this way, is always maintained in a truly vertical position, and no error can occur from a reading being taken with the tube held in a position varying from the plumb-line of direction. The instant the tube is brought out of the water the reading should be made by a telescope from a distance of a few feet, in lieu of which a magnifier may be employed, if the greatest expedition be observed.

"If this tube be graduated into 1000 parts, readings to one-thousandth part may be made with perfect ease.

"In taking coal-gas for examination by this tube, the dry tube should be filled with it, by displacing the atmospheric air with the gas to be examined. This is done by passing a small india-rubber tube, from which the gas is flowing, down the short limb *b* into the part *a*, and allowing at least five minutes (with 12 or 15-10ths of pressure) for its inflow, to ensure the expulsion of all traces of air. To fill the tube with water andt hen pass in the gas, might, by probably absorbing some of its constituents, lead to serious error, and this should never be done unless the supply of gas to be examined is so small that the waste consequent upon the use of the other proper method of filling the tube cannot be allowed."

The following passage occurs in Banister's "Gas Manipulation:" "In speaking of the condensation of the hydrocarbon vapours in coal gas by means of bromine and other agents, I ought not to omit to mention that there is a substance which will effect this object quite as successfully as bromine, and it is one which, at first sight, we should hardly expect to have this peculiar property. In the winter of 1860-1, Mr. Evans caused a naphtha box to be put up at the gasworks in Horseferry-road, for the purpose of improving the illuminating power of coal gas, when it was found that the photogenic power of the gas was very much decreased after passing through the naphtha, a circumstance which was at once attributed to the low state of the temperature, the thermometer at the time being below the freezing point; and, upon using naphtha instead of water in the eudiometer, it was found that the condensation was precisely the same as with bromine. Now, although it is quite clear that the naphthalising of gas depends partly on temperature, and partly on the mere mechanical effect produced by the gas passing over or through the naphtha, *yet the condensation of the hydrocarbon vapours by this means does not seem to depend upon temperature at all;* for, having made a great many experiments with the intention of setting this question at rest, I have invariably found that at temperatures ranging from 28° to 60° F. the condensation by naphtha has been identical with that by bromine, when the specific gravity of the naphtha was above ·800."

The part of this paragraph which relates to the naphthalising of gas I do not profess to understand, nor, I venture to think, did the author when he wrote it. What I am now concerned with is the statement that coal oils above ·8 in density, at temperatures below 60° F., remove from gas *all* its illuminating constituents. Though this statement is not made here *in words* it is made in substance, for the author had before said that bromine condensed all the illuminating compounds. If the condensation by coal oils be the same as that by bromine, the conclusion is that the oils condense all the luminiferous matter, and, therefore, that the gas after being passed through the oils gives no light. I knew that heavy oils would condense all the vapours of hydrocarbon oils which gas contained, but I doubted its power to condense gaseous illuminants, and experiment shows that it has not this power.



The experiments were made with most carefully purified oil of a greater density than water, at various temperatures, all of which were below 60° F., and with the annexed apparatus. A is a very large adapter, and B is a glass vessel capable of holding nearly three pints of fluid. These vessels were filled with sawdust saturated with the heavy coal oil, and some more oil was poured through the sawdust after the vessels were filled, so that it stood about an inch deep in B, but did not block up the inlet pipe P. The apparatus was shaken from time to time to supersaturate the sawdust in B; gas was passed through the apparatus at four feet an hour, and was burnt through a No. 3 Leoni's fishtail burner for common gas. The light of the gas equalled 1·3 candles : = ·325 candles per foot of gas. Here we see that *nearly all* the illuminating matter was dissolved, but not *quite all;* and if gas were to be valued thus, a wrong would be done to the company producing the gas to the extent of the illuminating matter not dissolved out. Another experiment was made with a 15-holed Argand, and the gas gave more light than in the fishtail.

TAKING THE SPECIFIC GRAVITY OF GAS.

Ascertaining the specific gravity of gas according to the methods generally given in books on gas, is an operation quite beyond the power of all but good manipulators, requiring the possession of a considerable amount of costly and delicate apparatus, and certain to lead to incorrect results, unless the experimentalist possess more knowledge than he can obtain from those books, and observe precautions of which they make no mention. One of the most recent and pretentious of these books gives Dr. Thompson's method of taking the specific gravity of gas, just introducing some figures and requiring the stopcock to be made of the new metal aluminium, because it is the lightest of all known metals! This piece of information is rather startling, for the specific gravity of aluminium happens to be 2·6, whereas that of the now well-known metal, magnesium, is but 1·7, and four other "known metals" are lighter even than this. What I am now concerned to point out is, that the author does not say one word about the elements of error to which the process is liable. Faraday, however, who wrote out of the fulness of knowledge, and whose skill as a manipulator is unrivalled, does this. Having quoted Thompson, he writes: "Notwithstanding the simplicity of the principle, however, much caution and even correction is necessary, without which it would be unsafe to recommend the process to the student. It is especially requisite that no gas of a preceding experiment remain in the globe; for which reason, after one experiment is finished, or before another has commenced, the globe or flask should be exhausted many times, air being admitted after each time; or, if access to the interior be easy, much air should be blown or drawn through it."

The air which is in the globe is the same as that of the atmosphere at the time it was introduced, and is therefore liable to all the variations to which the atmosphere itself is subject. Now, the weight of a given bulk of the atmosphere at the *same temperature and pressure* varies at different times, because of the variable proportions of water, and, perhaps, other substances which it contains.

It is not often saturated with moisture, and is never quite dry, and would require both hygrometrical experiment and calculation for a knowledge of its true state at any particular period. By reference to Daniell's tables of observation, it will be found that a difference in the dryness of the air, amounting to 20 degrees, of the hygrometer when the air was at the temperature of 60 degrees, has been observed within three days, there being at one time 1·053 cubic inches of vapour present in 100 cubic inches of the air, and at another 1·98 cubic inches: a difference of ·927 cubic inches of vapour (uncorrected for temperature) having occurred in that short period.

"If the air in the globe were dried, it would then be almost constantly the same in weight; for its two important ingredients vary very slightly if at all, and the difference in their weight is so small as to make these variations of no consequence."

"No choice can be permitted in this process. As to passing in the gas in a dry or a moist state, it must of necessity be dry."* Subsequently, Faraday gives directions for manipulating and obtaining accurate results when both gas and air are *saturated* with aqueous vapour.

The difficulty with coal gas is that we never know how much aqueous vapour it contains, and can ascertain it only by a separate experiment upon each sample of gas to be tested. What we call coal gas is not a gas, but a continually varying mixture of gases. These gases are not pure, but are always mixed with variable proportions of hydrocarbon vapours which differ day by day, according to the kind of coal used, its dryness, the temperature of the retorts, the kind and extent of purification, &c. Again, coal gas does not stand over *water* (except in the case of its being stored in a new gasholder), as is commonly supposed, but over exceedingly volatile hydrocarbon oils. When a gasholder has been in use for some time, the surface of the water within it is covered with a layer of hydrocarbon oils, condensed from the gas. The thickness of this oily layer varies with circumstances, but it is generally considerable. I know a case in which more than a thousand gallons were skimmed off a tank and sold. This layer of oils prevents or very much retards the evaporation of water from the tank into the gas, and the quantity of water carried away by gas from wet meters shows how far the gas is from being saturated with aqueous vapour, as it would probably be if it stood over water free from hydrocarbon oils.

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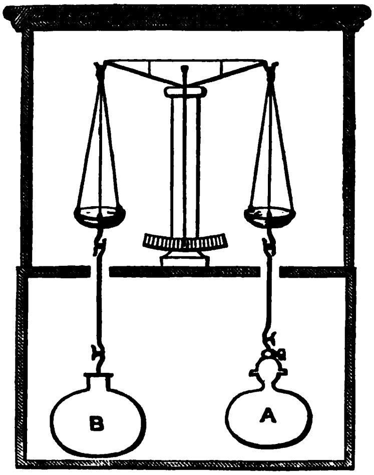
* Chemical Manipulation, pp. 394-5. Edition 1842.

My own determinations of the quantity of aqueous vapour in gas have not been sufficiently numerous to warrant a general statement; I therefore append the following, by Dr. Letheby: "The last impurity in coal gas is aqueous vapour (H O), the proportion of which varies with the temperature. It is rarely less than 200 grains, or about 1037 cubic inches, in 100 ft. of gas, and it rarely exceeds 600 grains, or 3111 cubic inches, in the 100 ft." Presuming that these figures are the result of experiment, they show us the importance of Faraday's statement, "*it must of necessity be dry.*"

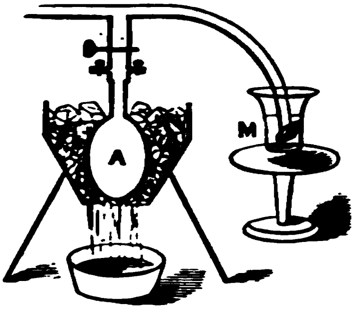
CHAPTER X.

GRAVIMETRIC METHOD OF TAKING SPECIFIC GRAVITY OF GAS.

A CLEAR and good description of the gravimetric process for taking the specific gravity of gases is given by Professor Miller in his "Elements of Chemistry," vol. i., pp. 197-8. Edition 1860: "The principal corrections required in the delicate operation of taking the specific gravity of a gas with accuracy have now been pointed out. Regnault, in his elaborate researches, has reduced the number of corrections ordinarily required, by counterpoising the globe in which the gas is to be weighed by a second globe of equal size made of the same glass; a practice which had previously been adopted by Prout, in his careful investigations on the density of the atmosphere. The film of hygroscopic moisture which always adheres to the glass is equal in both globes; and as the bulk of air displaced is also equal in both cases, the calculation for its buoyancy may be dispensed with. The following is a brief description of the method adopted by Regnault: A balance capable of weighing two pounds, and sufficiently sensible to turn with the $\frac{1}{150}$th part of a grain when loaded, is placed upon a chest provided with folding-doors, within



which the glass globes, each of the capacity of about 600 cubic inches, attached to the scale-pans, are freely suspended. The globe B is hermetically sealed; the globe A, for weighing the gases, is provided with a stopcock; the air is exhausted from A as perfectly as possible, and it is connected with an apparatus which supplies the gas to be weighed, the gas having been carefully purified and dried. The globe is again exhausted very completely, the last portions of air being thus displaced by the gas, and it is a second time filled with the gas: this process must be repeated a third time, and the gas will then be free from atmospheric air. To avoid the need for any correction for temperature, the globe is this time placed in a vessel of melting ice, in order to cool the gas to 32° F., which by the French is always taken as the standard. When the globe is filled with gas, and sufficient time has elapsed for it to acquire



the temperature of the ice, the vessel of mercury, M, into which the escape tube dips, is removed, so as to equalise the pressure within the globe with that of the air; the stopcock is closed and the globe withdrawn, wiped carefully with a damp cloth, to avoid rendering the surface electric, and it is then suspended to the scale-pan. It is not weighed, however, until after the lapse of a couple of hours, by which time the equilibrium of its temperature with the atmosphere is restored, and the production of currents around is obviated. The weight is then accurately noted; the globe is again plunged in ice, the gas removed by the air-pump, and the elasticity of the gas which still remains in it is measured accurately by the gauge attached to the air-pump. The empty globe is again withdrawn from the ice, and weighed as before; the difference of the two weights will give the weight of a bulk of gas, the elasticity of which is equal to that of the atmosphere, as marked by the height of the barometer H^1, diminished by the elasticity, *h*, of the remaining gas, as measured by the gauge. If the capacity of the globe has been previously accurately determined, the correct weight of the gas will be obtained by the following proportion:

The standard pressure.		The observed pressure.		The observed weight.	Corrected weight.
As H	:	H^1-h	: :	W^1	W''

WRIGHT'S BALLOON.

The late Mr. Wright endeavoured to substitute an easy method of taking the specific gravity of gas for the old and difficult one, and if his method had been accurate he would have conferred a favour upon all who have to perform the tedious operation which he tried to simplify. Unfortunately, the plan he proposed is entirely worthless. It takes no account of the carbonic acid and ammonia, and the very variable quantity of aqueous vapour in gas; and the capacity of the measuring vessel—a balloon gauged by a ring—is probably never the same for any two operations, and is, moreover, subject to endosmose and exosmose. As the balloon is in use, I think it right to mention it, and to give Mr. Wright's explanation of its use in his own words:

"There are several difficulties attending the attainment of a correct result from the specific gravity apparatus hitherto used by gas companies. These are:

"The small quantity operated upon; viz. 50 cubic inches.

"The weight of the flask, which is about 400 times heavier than the gas it contains.

"The want of a table for making the necessary corrections for the temperature and barometric pressure of gas, as well as air.

"These faults I have endeavoured to obviate by substituting for the flask a light balloon, capable of containing 1000 cubic inches,* and by calculating a table which, from the peculiarity of its construction, is applicable to the correction of the temperature and pressure of the gas, as well as air.

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* It has been found desirable to introduce a larger balloon, capable of containing a cubic foot, or 1728 cubic inches.

"*Directions for performing the experiment.*—Expel the air from the balloon, by folding it in the form in which it is first received; ascertain the weight of the balloon and car; fill the balloon with gas; insert the stopper, and put as many grains in the car as will balance it in the air; add the number of grains which it carries to the weight of the balloon, and deduct the amount from the tabular number corresponding to the degree of temperature indicated by the thermometer and the pressure indicated by the barometer; divide the result by the tabular number due to the temperature and pressure of the gas (to ascertain which, allow the gas to blow upon the bulb of the thermometer until the mercury is stationary), and the three first figures are the specific gravity."

I omit here an example worked out according to the tables, and add the "*Directions for using and preserving the balloon*," as they show how undeserving of confidence this process is:

"Weigh the balloon every time it is used, in order to allow for any slight increase in weight which may arise from oiling. Gauge the balloon, when inflated, by pressing the wire ring over it.

"The balloon is a little larger than the true size, to allow of its being inflated until the ring touches every part round it; when the ring does so, the balloon is sufficiently full.

"The balloon may also be filled by measuring a cubic foot of gas into it by an experimental meter.

"Take care that the balloon is not wetted, as it will be injured or destroyed if it is so.

"If the balloon become dry, and the gas is found to escape through the pores of the skin, the balloon should be blown full (taking care not to strain it), and a little oil, first spread over the palm of the hand, gently rubbed over it."

CONTINUAL INDICATOR OF SPECIFIC GRAVITY OF GAS.

"*A Continual Indicator of the Specific Gravity of Gas, by Drs. J. Bosscha and L. C. Ledoir, Leyden University.*

"Several gas engineers may have felt the want of an instrument of simple construction, serving to determine, by an easy experiment, the more or less valuable qualities of gas issuing from different kinds of coal, and from coals of the same kind subjected to different degrees of heat.

"Considering that the lighting power of gas, according to the experiments of Clegg and others, corresponds directly to its specific gravity, we tried a series of experiments with apparatus based on the principle just explained, but of different constructions, and at last succeeded in devising an instrument answering all the conditions required.

"This apparatus, simple in its application, may perhaps be found useful in gasworks. Its construction may be easily conceived by the following indications: A glass balloon, having a volume of about half a litre (= 30 cubic inches = 1 wine pint), is hung at one end of a balance with an inflected beam, such as is used for ascertaining the weight of letters. The accuracy and sensibility of this balance must be so great that variations in weight of one milligramme (= 0·01 English grain) make themselves visible on the circular scale, over which the longest downward inflected end of the beam moves.

"The whole apparatus is placed under a glass vessel, having an in-and-outlet for the coal gas, and standing in a pneumatic trough.

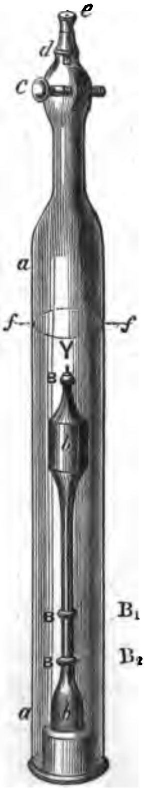
"It will be easily understood, that if the vessel is filled with gases of different specific gravities at the same degree of heat and pressure, the beam of the balance will assume a different position, and each position will correspond to a different specific gravity of gas. Our scale is divided into equal degrees, and their value fixed by filling the vessel first with pure hydrogen and afterwards with air. The weight of the glass balloon must be as low as possible. The one we made use of had, together with the hook, a weight of no more than 45 grains.

"It seems not difficult for micro-chemical glass apparatus makers to blow balloons of the proportions just described, but they have the disadvantage of being very fragile."—*Chemical News*, vol. i., p. 111.

BUNSEN'S METHOD OF TAKING THE SPECIFIC GRAVITY OF GASES BY EFFLUX THROUGH A FINE OPENING IN A THIN PLATE.

"It often happens that only a few cubic inches of gas are placed at the disposal of the analyst. The amount of material remaining after the necessary analyses have been made is, therefore, often insufficient for the determination of the specific gravity of the gas, according to the process just described (weighing). In such cases I employ another method, which gives results of sufficient accuracy, even with two cubic inches of gas.

"This method is based on the fact that the specific gravity of two gases, which stream out of a fine opening in a thin plate, are very nearly proportional to the square of the time of effusion. If a gas of a specific gravity, S, requires the time T, and another gas of a specific gravity, S_1, requires the time T_1, the relation between the times of effusion and the specific gravities is represented by the equation $\frac{S_1}{S} = \frac{T_1^2}{T^2}$. If S, or the specific gravity of one gas be made equal to one, the specific gravity of the other is found from the formula $S_1 = \frac{T_1{}^2}{T^2}$.

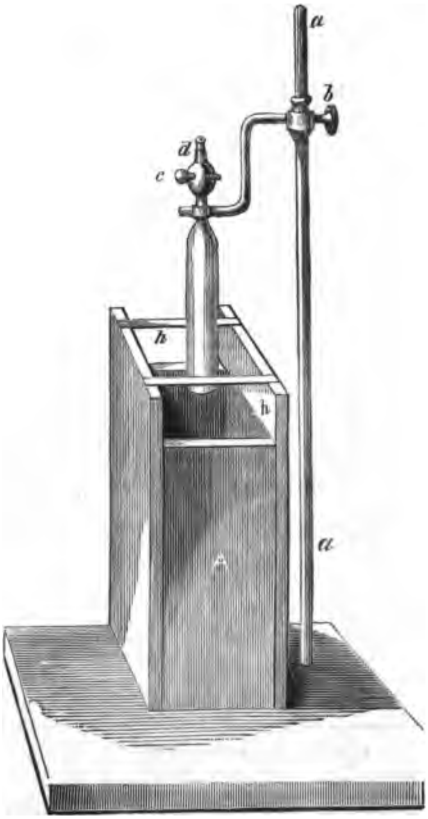


The annexed diagram represents the apparatus which is employed for these determinations. The glass tube *a a*, of about 70 cubic centimetres capacity, and open at bottom, is furnished with a glass stopcock at *c*, into which the small glass tube *e* is ground air-tight at *d*. This small tube is closed at the upper end by a thin piece of platinum-foil melted on to the glass, and pierced by a very small aperture. In order to render the foil as thin as possible, and the opening extremely small, a hole is bored through the metal with a fine needle, and the platinum, thus pierced, beaten out with a polished hammer on a steel anvil, until the whole is not perceptible to the ordinary eye, and is only just seen when the foil is held close between the eye and a bright flame. The foil is then cut into a small round disc, in the centre of which is placed the fine aperture. This small disc of metal is easily melted on the upper end of the small tube *e d*, by laying it on the blown-out end of the tube, and allowing the edges of glass to fall together over the metal by heating the tube in the blowpipe flame.

"In order that the gases under examination should issue from the aperture *e*, under precisely the same conditions of pressure, a float *b b*, made as light as possible of thin glass, is placed in the tube *a a*. This float carries at B a small bead of black glass, to which a thread of white glass is attached; and at B_1 and B_2 are placed two other threads of black glass, which, like the black bead, serve as marks of level.

"If the tube containing the gas to be examined, and the glass float, be dipped so deep in mercury that the level of the mercury outside coincides with a mark Y on the tube, the float is not visible to a telescope directed on to the mark Y. The stopcock *c* can now be opened, and the gas thus allowed to escape through the aperture *e*, so that the float *b b* rises with the level of the mercury inside the tube. During this time the experimenter must observe the level of the mercury through the telescope, and after a little time the white thread appears, giving notice that before long the bead will rise to the level *f f*. At the moment when the bead becomes visible, the observations of time must be made with a pendulum vibrating half-seconds, previously verified by a chronometer. These observations of time are concluded at the instant the black thread B_2 appears in the field of view of the telescope. The thread B_1 gives, as before, warning as to the end of the experiment.

"By means of these observations, the time of effusion of a column of gas is obtained, having a constant length of from B to B_2, reckoning from Y on the tube, and issuing under pressures the sum of which remains always constant. This time of effusion, determined for various gases, raised to the square, gives the relation of the specific gravities of the gases.



" The arrangement represented in the annexed diagram serves to hold the instrument. The tube is fastened to the arm *b*, which is movable on the standard *a a*. By means of this arm the tube can be sunk into a hole in the block A, until, when the stopcock is opened, the mercury completely fills the instrument. As soon as the tube is filled with mercury it is raised out of the mercurial trough, and, as the glass float is already contained in the tube, the gas is allowed to enter, in the usual manner, from below. If a large amount of gas is placed at the disposal of the experimenter, it is more convenient to remove the little glass tube *d*, and to allow the gas to enter the instrument from above, and to expel the air by the lower end of the tube, which dips under the surface of the mercurial trough. The arrival of the marks on the float, above the level of the mercury, is observed through the plate-glass sides, *h h*, of the trough. For the sake of greater accuracy, it is advisable to take the mean of several series of observations. It is scarcely necessary to mention that the gases must be employed in the dry state, and that all oxidation of the mercury, which would retard the motion of the float, must be most carefully avoided.

"The following experiments show the degree of accuracy which can be attained by this method. The first column, *t*, contains the times of effusion of a volume of air; the second column, t_1, the times of effusion of an equal volume of gas; the third and fourth columns, the square of these observed times; and the fifth column, the specific gravities calculated from these squares:

Air. t	Hydrogen. t_1	t^2	t_1^2	$\frac{t_1^2}{t^2}$
105·5	29·7	11130	882·09	0·0792
105·0	30·0	11025	900·00	0·0816
105·5	29·5	11130	870·25	0·0782
105·6	29·3	11151	858·48	0·0770
105·5	...	11130	...	

Air. t	Oxygen. t_1	t^2	t_1^2	$\frac{t_1^2}{t^2}$
102·5	108·5	10506	11772	1·1205
103·0	109·0	10609	11881	1·1199
102·8	108·5	10961	11772	1·1140

Air. t	Carbonic acid. t_1	t^2	t_1^2	$\frac{t_1^2}{t^2}$
102·7	127·0	10547	16129	1·5292
...	127·5	...	16257	1·5414

Air. t	Electrolytic detonating gas. t_1	t^2	t_1^2	$\frac{t_1^2}{t^2}$
117·9	75·4	13900	568·52	0·4090
117·0	75·5	13689	570·03	0·4164
117·9	75·5	13900	570·03	0·4101
117·6	75·6	13830	571·54	0·4133
...	75·9	...	576·08	0·4166

Air. t	1 vol. carbonic oxide + 1 vol. carbonic acid. t_1	t^2	t_1^2	$\frac{t_1^2}{t^2}$
117·9	130·5	13900	17030	1·2251
...	127·0	...	16129	1·1603
...	130·5	...	17030	1·2251

"The mean specific gravities, calculated from these experiments, are collected in the following Table. The first column contains the experimental results; the second column, the same values calculated from the atomic weights:

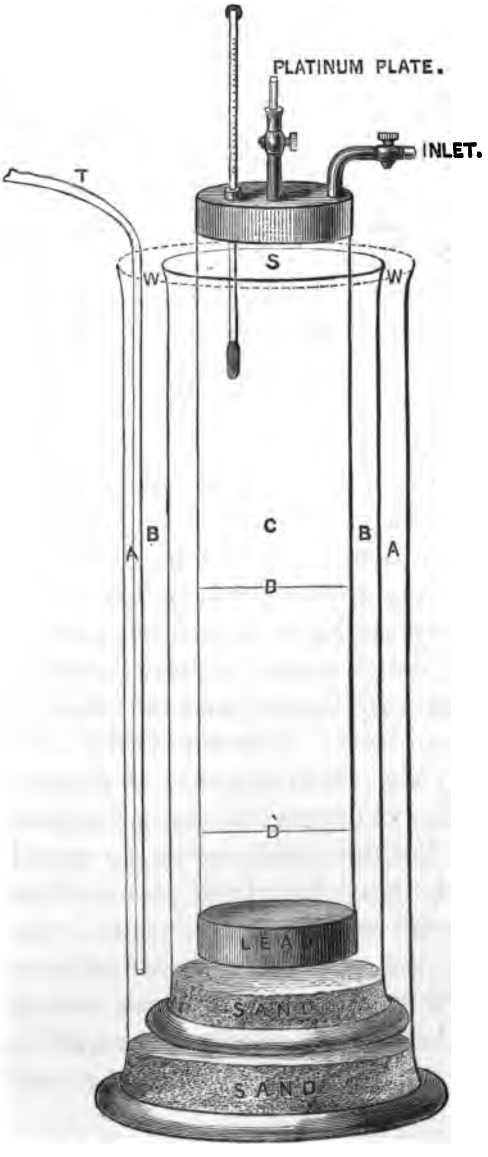
Gases.	I.	II.	Difference.
Air	1·000	1·000	—
Carbonic acid	1·535	1·520	+ 0·015
1 volume carbonic oxide + 1 vol. carbonic acid	1·203	1·244	- 0·041
Oxygen	1·118	1·106	+ 0·012
Electrolytic detonating gas	0·414	0·415	– 0·001
Hydrogen	0·079	0·069	+ 0·010

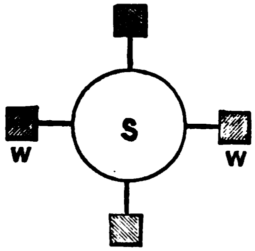
"It is seen that the agreement between the experimental and calculated values is very close. For technical purposes, as, for instance, the determination of the specific gravity of coal gas, this method is peculiarly applicable from its extreme simplicity."

This method is all Bunsen says of it as to simplicity; but, as he left it, quite unsuited for the majority of those who wish to take the specific gravity of coal gas. It involves the very costly fluid, mercury, which must be kept quite free from oxidation; a mercurial trough; a telescope and desiccating apparatus, and a pendulum apparatus, — things which imply a well-appointed laboratory. Moreover, the manipulatory skill necessary to handle tubes filled with mercury is by no means common among those who would gladly use an apparatus of small cost and requiring little skill. To meet this want, I constructed an apparatus which any one can use, and which, in point of cost, is within the reach of most; not to say that any one can construct one for himself for a few shillings. The principle is exactly the same as Bunsen's.

A NEW APPARATUS FOR TAKING THE SPECIFIC GRAVITY OF COAL AND OTHER GASES BY EFFLUX THROUGH A FINE OPENING IN A THIN PLATE OF METAL.

This apparatus consists of three cylindrical glass vessels placed one within another, C, B B, A A. The two outer ones are jars open at the top, and intended to hold water. The outer is 12·75 in. high, and 3·75 in. in diameter; the second 11·25 in. high, and 2·75 in. in diameter; the third and central one is a tube 14 in. long and ·75 in. in diameter. This tube is open at the lower end and closed at the upper by a good cork, S, through which are passed two small tubes of about 3 in. long and ·2 in. in diameter. The cork is cemented into its place, and afterwards is well covered by cap cement kept melted for a minute or two by a hot iron to prevent the possibility of any small holes remaining in it. One of the small tubes carries a tap, in which is fixed one end of a small glass tube, upon the other end of which tube a piece of pierced platinum foil is fixed by fusion of the glass. The second of the two small tubes is bent at right angles and carries a short piece of close-fitting india-rubber tube closed by a good clamp. By this latter tube the gas to be examined is admitted into the apparatus, and by the former it flows forth freely into the atmosphere. The lower and open end of the gas tube, C, is expanded at the blowpipe to a trumpet shape, and wrapped round with a ring of lead, heavy enough to sink it and keep it steady in its place when it is full of gas. The lead is covered with cap cement. Two easily-read horizontal marks, D D′, are etched or made with a file or a diamond upon the gas tube, and when this is done the apparatus is ready for use. A little clean sand in the inner glass jar forms a firm bed for the gas tube to rest upon. The gas tube slides in a metallic ring, S, figured on p. 238, which carries four arms fixed to four cork wedges, which fasten the ring securely within the top of the second cylinder, B B.





To use the apparatus, water is allowed to run into the outer glass cylinder through a tube, T, and a small stream is kept running into and out of it during the experiment. This secures uniformity of temperature. The interior jar which contains the gas tube is filled with water, through which has been passed a stream of the gas to be experimented upon. This expels air from the water, and prevents absorption by the water of any of the constituents of the gas during an experiment. The gas tube is filled with water by opening the tap or clamp and depressing the tube. When the tube is full the tap or clamp is closed, the tube is raised, and gas is passed into it from a tube, whence it is streaming freely. If any air have entered with the gas under examination, the clamp on the inlet should be shut, the tap on the exit tube opened, and the whole be blown off into the atmosphere. A second filling secures entire freedom from air. It is necessary to raise the gas tube while filling it, and to pass into it as much gas as will keep the lower of the two horizontal marks upon it *above the water* when the gas tube is in its place. When it stands firmly upon the sand the tap on the outlet tube is opened, and the gas, being under pressure, directly begins to flow out through the small hole in the platinum plate. As the gas passes out, the water rises in the tube. The rise of the water is watched with the eye parallel to the lower mark, D′, on the tube, and the time is noted at which it reaches it. The time is again noted when the water reaches the upper mark, D, and the difference of these times gives the period occupied by the gas in its efflux. Subsequently, the tube is filled with air, and the time of efflux of so much as is included between the marks on the tube is noted as was done with the gas. The specific gravity of the gas is found by dividing the square of the time of efflux of the gas by the square of the time of the efflux of the same bulk of air, thus:— $\frac{(\text{Gas time})^2}{(\text{Air time})^2}$ = specific gravity of gas. An example will make this clear. Air flowed out of an apparatus in 269 seconds: coal gas flowed out in 180 seconds. $\frac{(180)^2}{(269)^2} = 0{\cdot}447$ = specific gravity of the gas.

The accuracy of the instrument may be shown by a series of experiments on olefiant gas, which was made and purified with great care. The true specific gravity of this gas is 0·981; the specific gravity found by my apparatus on a mean of 10 experiments was 0·978.

Its accuracy is also shown by its power of detecting impurities, which alter the specific gravity of gases. Hydrogen prepared from commercial zinc and sulphuric acid was examined in a series of six experiments, and the specific gravity was found to be 0·0985, whereas the specific gravity of *pure* hydrogen is 0·0695. The impurities known to be present in hydrogen, prepared as mine purposely was, are shown by the increased specific gravity, as they should be if the instrument were reliable.

It is well to keep the platinum plate covered with a glass cap, as the dust of a laboratory soon clogs it, if it be left exposed.

PROUD'S METHOD OF TAKING THE SPECIFIC GRAVITY OF GAS.

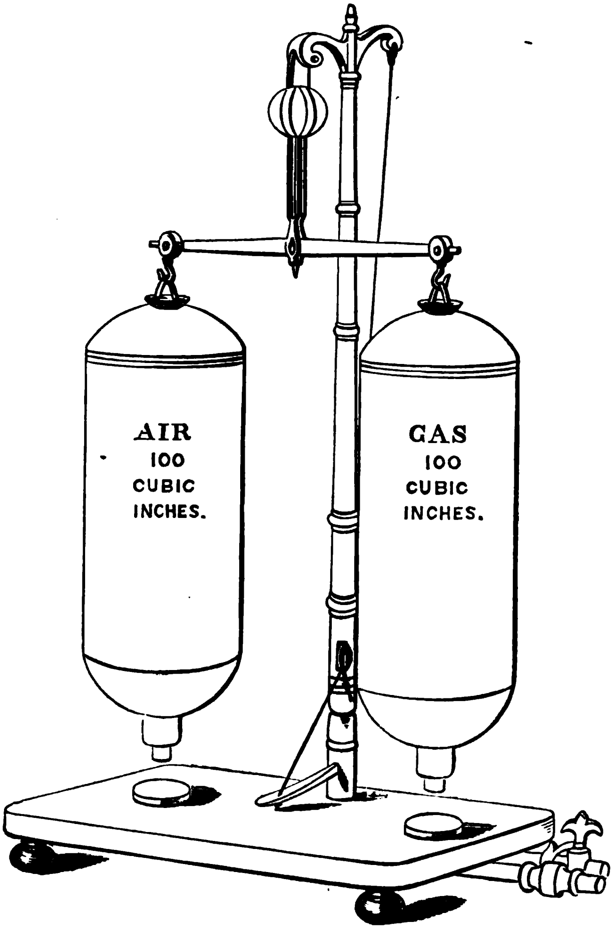
In 1862, Mr. Glaisher ascended in Mr. Coxwell's balloon from Wolverhampton, and it became necessary to ascertain the specific gravity of the gas with which the balloon was filled, in order that it might have its full ascensional force. The apparatus used to ascertain the specific gravity of the gas was published by Mr. Proud. I have given his own description of it. I may add that Mr. Glaisher informs me that it answered perfectly well for showing what was required for the balloon ascent, but whether its indications are scientifically exact I am unable to say, without testing the apparatus in use :

"The apparatus indicates the gravity of gas as quickly as gold may be weighed in an ordinary balance. The gas cylinder has an inlet and an outlet tube; the inlet passing through the neck below, and continued through the cylinder nearly to the crown, thus allowing the gas to be at once delivered to the upper part of the cylinder, where, owing to its lightness, it displaces the air, which is then expelled through the outlet at bottom of cylinder, and in a few seconds of time all trace of air is gone.

"The board which supports the balance is provided with a vulcanised india-rubber cushion, perforated for inlet and outlet; and when the cylinder is down, and the balance at rest, the weight of the cylinder pressing on the india-rubber makes the joint gas tight. The gas then makes its circuit, on the cocks being opened, through the inlet to the top of the cylinder, returning through the outlet at bottom, and thence conveyed by

⅜th copper pipe to the external air outside of the building.

"In manipulating with this instrument, there is no necessity for touching the cylinder with the hand, nor of removing it from the balance from which it is suspended. The whole operation resolves itself into turning the cocks and weighing; and correct observations may be taken almost instantaneously."



CHAPTER XI.

CARBURETTING OR NAPHTHALISING GAS.

Carburetting gas signifies adding to it vapours of hydrocarbons, which contain a large proportion of carbon in comparison of their other constituent, hydrogen; and as coal naphtha was first used for this purpose, and is yet the chief substance employed, the process was named naphthalising, from the fluid employed. Coal naphtha is a mixture of hydrocarbons, and is made up of a solid (carbon), and a gas (hydrogen), which, when united, form a fluid that contains a far higher per-centage of carbon than gas does. It is to this high per-centage of carbon it owes its use in improving the light of gas, to which its vapour is added. In fact, the light-giving power of gas supplied to us for combustion is largely due to the presence in it of vapours which, when they are condensed into a fluid, we call naphtha. We need not be chemists to ascertain that naphtha is much richer in carbon than coal gas is; for if we light gas it burns with a luminous but not with a smoky flame, whereas if we apply a light to a little naphtha in a saucer, it burns with a red flame, which affords but little heat, and deposits large quantities of its carbon in the well-known form of soot. The proportion of carbon is so large that there is not heat enough to burn it, and hence the smoke. The cause of this is obvious. When the naphtha vapour is fired both of its constituents burn, but the heat-producing constituent, hydrogen, is relatively so small in quantity that its heat cannot raise the carbon to the temperature required for its union with oxygen, or, as it is popularly called, burning, and therefore the unburnt carbon is deposited in visible black particles, instead of being converted into an invisible gas (carbonic acid), as it would have been had the hydrogen been in sufficient quantity to heat up the carbon to the temperature requisite for its combustion. Had this been the case, the flame would have been smokeless instead of smoky, and white instead of red. The case of gas is very different. When that is lighted, much more than enough heat is produced to burn its carbon perfectly. The richness ot naphtha in carbon may also be inferred from the fact that while a cubic foot of common gas weighs but about 224 grains, a cubic foot of naphtha weighs more than 370,000 grains. Now, this naphtha is composed of carbon and hydrogen only. Hydrogen is the lightest substance known in nature, and a cubic foot of it weighs but 37·1 grains, from which it is easy to see that the other constituent (carbon) gives the weight to the naphtha. Why is it necessary to add to gas these highly carburetted bodies? Because the solid carbon which they contain is required to give light. A gas which does not contain a solid gives but a mere trace of light, however great the heat produced by its combustion. This is well seen in the case of the gas hydrogen. When it burns, the heat produced is very intense. Solid substances held in the flame become strongly ignited and afford light, but without the solid the burning gas gives so little light that we cannot imagine a flame sufficiently large to afford us useful illumination; and, if we could produce such a flame, the heat of it would be unbearable. Hydrogen does not contain a solid, and the sole product of its combustion is water. If we pass hydrogen over naphtha, we find by the smell that it has carried with it a quantity of the vapour; and if we then burn the hydrogen so mixed with vapour, we obtain light, and the light increases as the quantity of vapour mixed with the hydrogen increases. In this case the hydrogen burns as soon as it comes into contact with the air, and produces intense heat. This heat decomposes the naphtha vapour, and sets free its carbon in the solid form, and then heats it to whiteness, at which temperature it affords light: the quantity of light being regulated, first, by the quantity of carbon present in the flame; and, secondly, by the temperature to which the solid carbon is raised. The greater the quantity of carbon and the higher its temperature, the greater the light. Another change is observed to take place when the hydrogen is mixed with the vapour. The flame of hydrogen alone is small in comparison of the flame produced by the same quantity of coal gas, but if the hydrogen be passed over naphtha, and burnt at the same rate as before, the flame is considerably expanded by the white-hot carbon contained in it. That which occurs in this case occurs universally. An increase of luminosity is always attended by an increased size of flame when the gases compared are burnt at the same rate.

What has now been said applies to the case of coal gas thus: The principal portion of coal gas consists of heat-producing substances, which do not give light, and which, like hydrogen, require to be mixed with bodies rich in carbon before they can be made useful. In gas itself we find some of these richly carburetted bodies, but their quantity is small even in the best gas, and of course much smaller in gas of inferior quality. Professor Bunsen, of Heidelberg, has published an analysis of Manchester gas, in which he shows the percentage of illuminants and non-illuminants; and although the Manchester gas is made principally from cannel coal, and has an illuminating power of from 20 to 22 sperm candles, for a consumption of 5 feet per hour, the quantity of non-illuminating gas is far larger than is suspected by persons not familiar with the subject. The following is Professor Bunsen's statement ("Gasometry," p. 113):

Analysis of Manchester Gas. In 100 *parts.*

Non-illuminating heat producers.	Hydrogen	45·58	87·12 per cent.
	Marsh gas	34·90	
	Carbonic oxide	6·64	
Illuminants	ELAYL	4·08	6·46 per cent.
	DITETRYL	2·38	
Impurities	Sulphuretted hydrogen	0·29	
	Nitrogen	2·46	
	Carbonic acid	3·67	
		100·00	

This is cannel gas. In common gas, such as that of London—which has little more than half the illuminating power of Manchester gas—the proportion of illuminants is smaller, and of non-illuminating heat-producers is larger. The mean of results obtained by many chemists shows that common 12-candle gas does not contain more than 4 per cent. of illuminating constituents, the other 96 per cent. being non-illuminating heat-producing compounds; that is, 4 feet in a hundred yield light, and 96 feet yield heat. The reader will see the manner in which the proportion of light-yielding compounds is generally ascertained by referring to p. 206. It seems extraordinary, that when we buy and burn 100 cubic feet of cannel gas, we obtain and use only 6½ feet of useful illuminants, and are obliged to take 87 feet of gases which do not furnish any light. An unscientific person is apt to exclaim, "Can this be? And if it be, is it necessary? Cannot the illuminants be made and sold without the heat-producers?" This is impossible. Gas cannot be made without its bulk being non-illuminating. But still something can be done to *increase the proportion* of illuminants, and this is what is called naphthalising or carburetting. We find by experiment, if we pass gas over the highly carburetted substances called naphtha, that every foot of gas carries away with it as vapour a part of the naphtha; that the naphtha vapour burns with the gas; and that the light-giving power of the gas increases with the quantity of naphtha vapour so removed and burnt. We can measure the gas, weigh the naphtha removed, determine the increase of light obtained, and thus ascertain exactly the illuminating value of each grain of naphtha burnt with the gas. The naphtha, thus added to gas, does not burn with a red flame, which gives off much smoke, as it does when burnt alone; but it gives a bright, white, smokeless flame. This result is due to the non-illuminating but heat-producing compounds of the gas. The naphtha contains so much carbon and so little heat-producing hydrogen, that when it is set on fire alone the carbon is not heated sufficiently to burn, it still less to render it highly luminous; wherefore its light is feeble, and much of the carbon is deposited as soot. But when it is burnt with gas the heat-producers in the gas raise the carbon of the naphtha to a sufficient temperature to enable it to give off a brilliant light, and to burn without smoke. Thus the heat-producers are rendered directly useful. They raise the carbon to the temperature required for illumination and combustion. The naphtha, on the other hand, remedies the want of luminosity in the gas by adding to it much carbon, in which it is deficient, and but little hydrogen, in which the gas itself abounds.

But for the teaching of experiment, we should expect that if a given quantity of naphtha added to a given quantity of gas raised it in illuminating value (a) candles, then double the quantity of naphtha added to the same quantity of gas would raise it ($2\ a$) candles, three times the quantity ($3\ a$) candles, and so on. In other words, we should expect the illuminating value of a grain of naphtha to be the same, however it was burnt with gas. Experience shows this to be a mistake, and proves that the illuminating value of a grain of naphtha depends upon *the relation which it bears to the bulk of gas with which it is burnt*, and that, within certain limits, its illuminating value varies greatly.

Dr. Letheby investigated the increase of illuminating power obtained by naphthalising, and his results are published in the *Chemical News*, vol. vi., pp. 283, 292. It is there stated that 4 grains of naphtha added to a foot of gas gave 25 per cent. increased light, whereas 8 grains per foot gave 60 per cent. increased light, instead of 50 per cent., which would have been the increase if each grain of carburetting material had a constant illuminating value irrespective of its relation to the bulk of gas with which it is consumed. The investigation had reference to the use of naphtha in the public lamps of the city of London, and therefore the results must have been obtained in flat-flame burners, bat's-wing or fish-tail. The average illuminating value of a foot of common gas, burnt in a flat-flame burner, is 1·6 candles, and the weight of a foot of such gas is about 224 grains. When the relation between the weight of the gas and the naphtha burned with it is as 224 to 4, the illuminating value of the naphtha is 25-100ths of the illuminating value of the gas; but when the relation is altered to 224 to 8, then the illuminating value of the naphtha is 60-100ths instead of 50, as it would have been if its illuminating value were constant instead of variable, and depending much upon the quantity of gas consumed with it.

During the long series of investigations which resulted in my discovering a practicable means of removing sulphur from coal gas, I was compelled to examine many samples of naphthalin, as part of the sulphur in gas was always found in chemical combination with that substance. The large proportion of carbon in naphthalin pointed it out as peculiarly fitted to yield much light with little heat, if it could be burnt, and from time to time attempts were made to burn it. All resulted in failure, and in 1860 I was led to join in the general opinion of chemists, that it could not be burnt. Subsequent experiments pointed out to me some of the causes of failure, and led to a reinvestigation of the subject, which resulted in my ascertaining the conditions which must be complied with in order to burn naphthalin advantageously as an illuminant, and in the construction of an apparatus, which has since been extensively used, and produces the finest light which has yet been seen from gas. The principles of this process are so important, that it will be worth while to explain it somewhat in detail. At ordinary temperatures naphthalin is a white, crystalline, volatile solid, which emits a vapour having the smell of the narcissus. If allowed to be freely exposed to air, it volatilises in time like camphor. The crystals melt to a nearly colourless fluid at 174° F., and when melted much more vapour is given off than at lower temperatures, which allow the substance to crystallise and remain solid. At 428° F., naphthalin boils and distils over, and when cool enough resumes the crystalline condition. The small amount of latent heat of its vapour, and the ease with which it is cooled and made to resume its crystalline form, is a source of considerable difficulty in its use, for it blocks up the gaspipes, and stops the flow of gas whenever it is used in improperly-constructed apparatus. A good illustration of this is furnished by the street mains, which are frequently blocked up in winter by naphthalin, so that no gas can pass through them. The naphthalin was in a state of chemical combination, as vapour, when it left the works to pass through the mains; it was there cooled below the temperature at which its feeble chemical combination could exist, and consequently it was liberated. At the instant of its liberation it obeyed the law of its nature and crystallised, and so stopped the pipes. This is a fertile source of trouble and annoyance, and the best and most careful gas engineers are liable to it as well as the careless, if a very high temperature be employed in producing the gas. Experiment proves that ordinary gas at common atmospheric temperatures has no power to unite with naphthaline, which at those temperatures is of course in the crystalline state. I have passed coal gas through nearly three feet of crystalline naphthaline, and have not found the illuminating power of the gas altered a quarter of a candle. If the naphthaline be heated so as to convert it into a fluid, the result is widely different. Then the gas carries with it a large quantity of the naphthalin vapour, and if this be carried to the burner, a great increase of light is obtained. The combination between the gas and the vapour is so feeble, that if they be passed through tubes at common temperatures, the naphthalin resumes the crystalline condition, and no increase of light is observed at the photometer.

DESCRIPTION OF APPARATUS.

The apparatus I employ consists of a gas-tight metallic vessel to hold the hydrocarbons, from which depends a gaspipe carrying one or more burners. The hydrocarbon vessel is provided with a screw-plug for convenience of filling, and with an inlet for gas, by which it can be attached to ordinary fittings. The hydrocarbon vessel is placed above the gas-flame, so that it may be heated thereby to the temperature required to enable the gas in its passage to combine with and carry forward a suitable quantity of hydrocarbon vapour. Seven inches between the burner and the hydrocarbon vessel are found in practice to be the most suitable distance to secure enough, but not too much, vapour. When the gas is first lighted it gives but little light, owing to the fineness of the burners through which it passes; but as its heat warms the hydrocarbons, their vapour passes with the gas which sweeps over their surface, and the light gradually improves until the full illuminating power is attained. This happens as soon as the hydrocarbons have attained their normal temperature. The vapour of these hydrocarbons is more than four times the density of air, and consequently their tendency is to descend. Their specific heat is so small that they condense very readily, and the union between them and the gas is dependent upon the maintenance of high temperature; wherefore I place the pipe which carries the burners below the hydrocarbon vessel, that the flow of heavy vapour may be facilitated by gravity. This pipe, moreover, is so placed as to be heated considerably, so as to prevent condensation, and thus is secured an illumination which cannot be procured otherwise. The light is so placed in reference to the pipes as to be shadowless. In some situations this form of apparatus is unsuitable, and I have therefore adopted a modified form suitable for chandeliers, &c., but still retaining the principle of heating the hydrocarbons, and passing them and the gas through heated tubes. In gaseliers the hydrocarbon vessel is heated to the proper temperature by a small subsidiary jet of gas, and the arms which carry the burners extend from the vessel but a very short distance, so that they may be kept hot enough to prevent condensation.

Another mode of exhibiting the subject is to show the amount of light obtained for a given sum of money, and then to show the quantity of material consumed to give that light.

Carbolene is the commercial name given to heavy oils which are employed in my apparatus for carburetting gas. It is sold for 2s. 6d. a gallon, weighing 10 lbs. Like all fluid hydrocarbons this is a mixture, but as it differs very little from naphthalin, of which I have already spoken, and naphthalin is a definite chemical compound, I shall take it as a typical substance. Taking its commercial value, then, to be the same as carbolene, and finding its illuminating value when burned to enrich gas to be one candle for a consumption of five grains, we have 14000 candles' light as that yielded by naphthalin which costs 2s. 6d., and weighs 10 lbs. or

70000 grains. Taking gas at 4s. 6d. a thousand feet, and its illuminating value as 1·6 candles per foot, we have 555 ft. as the quantity purchasable for 2s. 6d., and the light yielded thereby equal to 888 candles. Five hundred and fifty-five feet of gas weigh 124320 grains, so that we must burn this weight to produce the light of 888 candles at a cost of 2s. 6d. The weight of naphthalin required to give the same light is 4440 grains. Tabulated on the basis of the light given by gas which costs 2s. 6d., the result will be:

	Cost.	Light.	Weight burnt to obtain light.
Gas	2s. 6d.	888 candles	124320 grains
Naphthalin	0 2d.	888 „	4440 „

Or the result may be shown thus:

Naphthalin, illuminating value 5 grains = 1 candle.
Gas „ „ 140 „ 1 „

	Weight burnt.	Light.	Cost.
Naphthalin	10 lbs.	14000 candles.	0*l.* 2s. 6d.
Gas	280 „	14000 „	1 17 4

These facts and figures show the economy of carburetting gas in the most marked manner, and no comment would be required, if my purpose were merely to manifest the economy of light from carburetted gas, as contrasted with that obtained from gas alone. I wish, however, to call especial attention to the explanation of another point furnished by these calculations, which is almost of as much moment as economy: I refer to the *smaller quantity of heat* produced by carburetted gas, as compared with that produced by uncarburetted gas, to afford the same quantity of light.

LESS HEAT FOR A UNIT OF LIGHT FROM CARBURETTED GAS THAN FROM UNCARBURETTED GAS.

To obtain the light of 888 candles, there must be burnt

124320 grains of gas, whereas this light is obtained by the burning of 4440 grains of naphthalin, when this substance is added to and burnt with gas to increase its illuminating power. If the heating power of gas and naphthalin were equal, weight for weight, the gain by carburetting would be very large; but when it is remembered that the heating power of an equal weight of common gas is much greater than the heating power of the same weight of naphthalin, the advantage becomes still more striking. The enormous heating power of gas is not apprehended by the general public, though it has long been familiar to men of science. The following Table shows the comparative heating powers of equal weights of different combustibles, according to Despretz:

1 lb. of	HYDROGEN	heated 236 lbs.	of water from the freezing point to the boiling point.
„	Oil, wax	90	„ „ „ „
„	Ether	80	„ „ „ „
„	PURE CHARCOAL	78	„ „ „ „
„	Common wood charcoal	75	„ „ „ „
„	Alcohol	68	„ „ „ „
„	BITUMINOUS COAL	60	„ „ „ „
„	Baked wood	36	„ „ „ „
„	Wood holding 20 per cent. of water	27	„ „ „ „
„	Turf (peat)	25 to 30	„ „ „ „

We hence see that 1 lb of hydrogen in burning, produces more than three times the heat produced by 1 lb. of carbon in burning, and nearly four times the heat produced by burning 1 lb. of coal. It is easily seen from this how great is the heat given by the combustion of common gas, and how necessary it is to enrich gas by the addition of carbon whenever much light is required, if we would not render shops, offices, factories, and other hives of industry unbearably hot and dangerous to health. The best as well as the cheapest substances for this purpose are the heavy hydrocarbons obtained from gas tar, from among which I have selected naphthalin as a typical substance. These highly carburetted compounds utilise the otherwise useless heat producing portions of common gas, and give a maximum of light with a minimum of heat.

The average heating power of common London coal gas, made from Newcastle coal, is accepted as twice that of ordinary bituminous coal. The table quoted from Despretz shows that 1 lb. of coal will heat 60 lbs. of water, from the freezing point to the boiling point, so that 1 lb. of common gas having double the heating power will raise 120 lbs. of water through the same range of temperature. 100 lbs. of gas will of course heat 12000 lbs. of water from the same to the same point of the thermometric scale. 100 lbs. of naphthalin, on the other hand, will heat similarly but 8786 lbs. of water, so that for *equal weight*, the heating powers of gas and naphthalin are as 12000 to 8786. But experiment shows that 5 grains of naphthalin have the same illuminating value as 140 grains of gas; that is, that to obtain equal light we must burn 28 times the *weight* of gas which we burn of naphthalin. Wherefore as the illuminating equivalent of 100 lbs. of naphthalin, we must burn 2800 lbs. of gas; that is, we produce heat in the one case which will raise 8786 lbs. of water from its freezing point to its boiling point, and in the other case, heat which will raise 246008 lbs. of water through the same temperature!

The importance of this subject will be felt most by those who are compelled to pass many hours where a large quantity of light is obtained by the combustion of large quantities of gas. Nowhere, perhaps, is a good light of more importance than in the offices of a daily newspaper, and nowhere is the heat produced by gas complained of so loudly. The very large quantity of heat produced by the gas unavoidably burnt in the compositors' room of a daily paper, renders it almost unbearable to a stranger who enters it from the fresh air late in the evening, and no doubt much injury to health results from leaving such an atmosphere and passing out into the cold of a winter's night. The most competent witnesses to the detrimental heating effects of common gas may be found on the staffs of that great and wonderful organisation, the daily press; and no persons will more appreciate greater light and less heat than this most deserving class, to whom we are all so much indebted. I have seen and heard signs of pleasure in a newspaper office lighted by my carburetting apparatus, which would have appeared strange to a person unacquainted with the evil of the overheated atmosphere in which all had been compelled to labour hitherto, and from which they were delivered. It is impossible not to feel satisfaction at having contributed to lessen the inconveniences of those who toil when the world is wrapped in sleep, in order that when it awakes next morning it may have the means of becoming acquainted with the progress of men and things in this busy age.

LESS CARBONIC ACID PER UNIT OF LIGHT FROM CARBURETTED GAS THAN FROM UNCARBURETTED.

From what has been said already, it will be seen, that for equal lights, less carbonic acid is produced from my richly carburetted gas than from ordinary gas. Ordinary gas contains from 40 per cent. to 60 per cent. of marsh gas and carbonic oxide, which are non-illuminants, and therefore must be burnt uselessly as to light, although their carbon produces carbonic acid when burnt, just as if it were giving light. By lessening the quantity of gas consumed we lessen this unnecessary production of carbonic acid, and all the carbon added in the carburetting material yields light.

From another point of view, this may be seen even more clearly. Last year Mr. Anderson published some experiments to show that the "diluents" in gas, as they are generally called, are not simply useless as light-giving agents, but are "destructives," *i.e.* they actually destroy part of the light-giving matter in the gas with which they are mixed; so that if it were possible to withdraw these destructive compounds from a given quantity of gas, and to burn the remainder of the gas, more light would be obtained from the remainder than from the original bulk of gas containing the "destructives." He showed that the full destructive effect of these compounds upon the luminiferous constituents of the gas must be exerted before we obtain any light. From this it is clear that their destructive power is exhausted upon the light-giving matter of the gas in which they occur, and that if more light-giving matter be added to gas they cannot exert any detsructive power upon that, as the whole of their destructive power was expended upon the light-giving matter of the gas itself. It follows, therefore, that we obtain the full light due to any hydrocarbons we may add to gas, provided we add them in such quantity as to be raised to the proper illuminating temperature; in other words, in such quantity as to be burned without smoke.

This affords an explanation of what has appeared a great puzzle to many people, viz. how so much light can be obtained from an addition to gas of so small a quantity of rich carburetting material, and, consequently, how light can be produced from well-carburetted gas much more cheaply than from gas alone. One foot of gas enriched with 31·5 grains of naphthalin gives the light of five feet of gas as measured at the photometer, and it has been asked, how is it possible that 31·5 grains of substance added to the foot of gas can equal in light-giving power 896 grains of gas? The composition of gas and the destructive influence of some of its chief constituents fully explain the apparent anomaly. It is always satisfactory to be able to explain a fact, but it must be remembered that the existence of a fact depends upon evidence, and not upon our ability to explain it. The increased light I mention has been measured scores of times, by many independent observers, and this evidence proves it to be a fact. Its existence would not be affected if we were ignorant of its explanation.

Mr. Anderson's results are the following. He made a mixture of gases as represented below:

Marsh gas	38·5	parts.
Carbonic acid	2·0	„
„ oxide	11·0	„
Hydrogen	38·5	„
Nitrogen	4·0	„
Deficiency	6·0	„
	100·0	„

The "deficiency" here represents nearly the quantity of illuminating materials found by Bunsen in his analysis of Manchester gas.

This mixture burnt with a dull blue flame about two inches in length, and gave no light which could be measured against the candle.

He then passed coal gas through fuming sulphuric acid and potash to absorb the illuminating material, and found the remainder (of which he does not give the quantity or per-centage composition) identical in illuminating power with the mixture above mentioned; he then made the following mixture, which contains 10 per cent. of the highly illuminating gas elayl (*i.e.* olefiant gas), and found that this mixture gave no light; that, in fact, the 4 non-illuminating gases destroyed the light which would have been given by the 10 per cent. of illuminating gas. This mixture was:

Carbonic oxide	20·0
„ acid	2·0
Hydrogen	62·0
Nitrogen	6·0
Elayl (olefiant gas)	10·0
	100·0

He subsequently made the following mixture of gases, and was unable to obtain from them any light which could be measured by the photometer:

Marsh gas	60·0
Carbonic oxide	10·0
Hydrogen	30·0
	100·0

If these results be trustworthy, they prove—

I. That coal gas owes its illuminating value wholly to olefiant gas and vapours of hydrocarbons which Bunsen has comprehended in the term tetrylene.

II. That the non-illuminating portion of coal gas has the power of destroying a certain proportion of the light which its illuminating constituents would have yielded if they had not been mixed with non-illuminants.

Another of his results is that, when 1 per cent. of the illuminating hydrocarbons, always present in coal gas, is added to a mixture of non-illuminating gases, this addition of 1 per cent. adds to the lightless mixture as nearly as possible the light of 3·25 standard candles. That 2 per cent. of these hydrocarbons gives it an illuminating value of 6·5 candles, 3 per cent. 9·75 candles, and 4 per cent. 13 candles.

This is entirely opposed to universal experience, which shows that the illuminating value of hydrocarbons added to gas increases very much in proportion to the quantity added, and not directly as the quantity, which the above statement implies. Moreover, it seems impossible to reconcile this with the statement made by Mr. Anderson himself, that 90 per cent. of non-illuminants destroys the illuminating power of 10 per cent. of olefiant gas. Certainly that non-illuminating mixture would have given a *ten per cent. condensation with bromine*, and I am entirely at a loss to understand how the various statements can be reconciled.

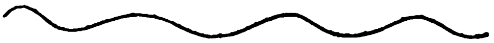
The superiority of carburetted over uncarburetted gas-light is shown—I. By the steadiness of the former as compared with the latter; and II. By the carburetted light enabling us to distinguish colours with almost the same distinctness and accuracy as we distinguish them by day-light. The imperfection of ordinary gas-light in the latter particular is well known.

I. Steadiness.—This property of gas-light cannot be obtained unless the gas is sent through the burner under some pressure, so as to strike against the air with force. The steadiest light is obtained when a burner is emitting nearly the greatest quantity of gas it is capable of passing without whistling. This renders the light much less than might be obtained from the same quantity of gas burnt through a burner of large aperture under a pressure of not more than one or two tenths of an inch of water. In burning gas, we can obtain steadiness at the expense of quantity of light, or quantity at the expense of steadiness, but we cannot have the two at the same time from ordinary gas. The following Table of Mr. Banister will show the variable quantity of light obtained by burning the same gas through burners with different apertures and under different pressures. The burners with the highest numbers have the largest apertures:

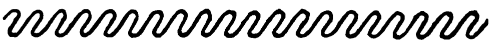
Burner.	Pressure of gas.	Illuminating power.	Value of one foot of gas in standard candles.	Quantity of gas consumed.
Bat'swing, No. 8	5·5 tenths.	9·4	1·88	5 feet.
" " 7	4·6 "	9·0	1·80	5 "
" " 6	10·5 "	8·0	1·60	5 "
" " 5	4·6 "	8·6	1·72	5 "
" " 4	9·7 "	5·7	1·14	5 "
" " 3	16·0 "	3·5	0·70	5 "
Fishtail " 8	3·5 "	12·1	2·42	5 "
" " 6	6·6 "	9·3	1·86	5 "
" " 5	8·8 "	6·3	1·26	5 "
" " 4	13·9 "	5·1	1·02	5 "
" " 3	18·0 "	2·1	0·50	4.2 "

Highly carburetted gas, on the other hand, cannot be burnt except through burners with small apertures and under considerable pressure, for if it be not driven against the air with considerable force, and in a thin stream, the carbon will not burn, and smoke and diminished light result. But these are just the conditions which produce steadiness, and thus, while an abundance of carbon ensures *quantity* of light, the fine burners and pressure employed ensure steadiness of light. Many persons who have seen lights highly carburetted by my apparatus, have compared them for steadiness to lights painted. In a still atmosphere it is hardly possible to detect a motion. The experience of most persons will tell them how different is the flickering light of common gas when burnt through large apertures under low pressures to obtain the maximum of light which it is capable of yielding. The undulations of the two lights may be represented graphically somewhat thus:

Common gas



Gas highly carburetted



Another property of highly carburetted gas is that by its light we can distinguish colours with almost the same ease, distinctness, and accuracy, as by daylight. My attention was first called to this subject by hearing that an artist who was using one of my lights was able to colour his drawings at night as easily as by day. The gentleman in question having assured me that such was the fact, I was induced to make many experiments upon the subject, the results of which fully bore out the statements of the artist. This becoming known, induced the committee of the Wakefield Fine Arts and Industrial Exhibition to light their two large and well-filled picture-galleries with my apparatus. The result was every way satisfactory to the committee, as well as to the scores of thousands of spectators who visited the galleries, for the pictures could be seen as well as by daylight.* I take this property of showing colours correctly to be the result of *quality* and not of mere *quantity* of light, and an indication that the richly carburetted light approaches more nearly to sun-light in quality than ordinary gas-light does. In fact, the spectrum of my light approaches the spectrum of sun-light more nearly than that of any other artificial light does. The importance of a good light to persons who work much by night and wish to preserve their sight, is well known, but the *quality* of the light is not much thought of. Yet, probably, quality has very much to do with the matter, and I shall be glad if experience prove that the nearer approximation to the quality of sun-light which is indicated by the definition of colours, be a means of preserving the sight of the increasing numbers who are obliged to prolong the day into the hours of darkness in order to obtain the subsistence or fame which are now competed for so fiercely.

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* This will be a great boon to persons engaged in the sale and manufacture of coloured articles, such as drapers, tailors, dressmakers, &c., as well as to artists.

Mr. Hughes's book on "Gasworks and Gas" has recently been republished with considerable alterations by Mr. Richards. Among other alterations, he has added a chapter on carburetting gas. After narrating a series of experiments to show the irregular manner in which hydrocarbon oils evaporate at ordinary temperatures, he says: "These experiments are highly instructive as regards the carburisation of gas; they show the great irregularity in the evaporation, also that a portion of the liquid will not evaporate with sufficient rapidity under ordinary temperatures, and they show that a portion does not evaporate. If, on the contrary, the liquid evaporated regularly, even although it might require a comparatively high temperature for the purpose, then the carburisation of gas would be a very valuable adjunct to gas-lighting, as it would give to ordinary gas all the advantages of cannel, producing less heat for a given quantity of light, securing better ventilation, and the agreeable glow produced by the light of rich gas, which is an advantage much to be desired."

The reader will perceive that the desiderata pointed out by Mr. Williams in this extract are exactly supplied by my process.

An experiment was made in the hall of the Royal Scottish Society of Arts, on the occasion of reading a paper on my process a short time since, which will show the superiority of well-carburetted gas to the best ordinary coal-gas which is made in the United Kingdom. The hall of the Society is lighted by three sun-lights, containing 25 burners each. To the centre of these sun-lights Messrs. Hogg, of Edinburgh, attached one of my carburetters, and stopped off five of the burners, so that there were 20 lights carburetted. Two out of the three sun-lights were extinguished, and the centre one was burnt with 20 lights, and then people read in every part of the room with perfect comfort. If the gas commonly used in that room had been common 12-candle gas, I should not have thought the experiment worth notice, as far more striking results are obtained every day with common gas; but when the best gas in the kingdom (probably in the world) can be improved thus, the experiment is worth a record, and such a success may be well accepted as a partial recompense for the labour expended in the investigations which led to the process.

Carburetting Materials.—The value of a technical process depends much upon its being economical as well as useful, and in this process it is indispensable that the carburetting materials be attainable in sufficient quantity for general use, and at a price which renders the light obtained thus cheaper than when obtained from gas alone. The principal substances I employ are cymole $C_{20}\,H_{14}$, and naphthalin $C_{20}\,H_{8}$, which are produced unavoidably and in enormous quantity as a bye-product in the manufacture of gas.

Neither of these substances has received any important application in the arts, and, consequently, they are obtainable, not only in sufficient quantity, but at a reasonable price. In fact, naphthalin might be called a waste product until I utilised it for carburetting gas. One author, speaking of part of the oil from tar containing much naphthalin, said it " is almost worthless from the amount of naphthalin it contains, and it may therefore be employed as fuel;" for which it was certainly worthless, as it cannot be burnt in any furnace constructed to burn coal. Properly used, that portion of the oil is a valuable illuminant.

Safety of Carburetting Material.—It is well known that coal naphtha gives off much vapour at ordinary temperatures, and that if a light be held near a vessel of naphtha this vapour will take fire, and will ignite the naphtha. Many serious accidents and fires have occurred from the incautious placing of lights near naphtha and paraffin oil, or petroleum. Combustible vapour is evolved at ordinary temperatures by all naphtha which will carburet gas without being heated for the purpose. It is impossible to find a sample of hydrocarbons which will not take fire from a naked flame held near it, and which will carburet gas at ordinary temperatures. The hydrocarbon fluids, of higher boiling point and higher specific gravity, on the contrary, evolve no appreciable amount of combustible vapour at common temperatures, and they are as safe and as incombustible as water, unless they be artificially heated so as to convert them into vapours, instead of fluids and solids; and, even when heated so as to become vaporous, *they will not burn alone*, but require the heating power of a combustible gas to sustain their combustion. Air saturated with benzole vapour can be consumed as a light-giving agent through a gas-burner, just as gas is burned. Petroleum vapour likewise will burn with air; but the vapour of heavy hydrocarbons, with a boiling point of 400° F. and upwards, and a specific gravity about the same as water, cannot be kept alight in the same way for a single minute. I have repeatedly heated these fluids to the temperature of boiling water, and, while so heated, have plunged lighted tapers, candles, paper, matches, &c., into them, with a uniform result. The lights were extinguished as they would have been by immersion in water, and in no instance were the hydrocarbons ignited. They have been allowed to leak over the carburetting apparatus while the gas was burning brilliantly below it, and they have not fired. In fact, their use and storage are attended with no more risk than the use and storage of olive oil.

This safety arises from the large quantity of carbon and the small quantity of hydrogen which they contain. Carbon is a non-volatile substance, and burns with difficulty. Hydrogen, on the other hand, is the most volatile substance we know of, and burns most readily. Substances rich in hydrogen—such as petroleum, paraffin oil, light coal-tar naphtha, spirit of wine, &c.—are volatile and easily inflamed; whereas substances poor in hydrogen—such as paranaphthalin, pyrene, and chrysene—are not volatile, and are inflamed with great difficulty. This combination of great safety, with the most brilliant artificial light known, is entirely new to science, and cannot fail to be appreciated in practice. Hitherto increased light involved considerably increased risk, and the insurance offices would not permit the use of light-carburetting fluids on account of their danger. After the most searching examination of the fluid I employ, they not only allow its use, but their officers state that they are unacquainted with anything in use as an illuminant which is so safe.

Mansfield's conclusions agree with mine. He found that oil which distilled at 230° F. will take fire at the surface when a light is applied to it; but it yields so small a quantity of vapour to cold air, that the air thus carburetted gives but a blue flame. Oil which distils at 300° F. is still less inflammable, for it cannot be fired at the surface, and it will not furnish any illuminating vapour to atmospheric air. Oil of this boiling point is therefore as safe as ordinary vegetable oils when stored and used. The oils used in my carburetting apparatus have a much higher boiling point than this, and therefore are still less inflammable; but, though less inflammable, they are much more illuminating when burnt.

The exhibition and explanation of my carburetting process in London, drew forth from the editor of *The Journal of Gas-Lighting* an article characterised by inaccuracy and gross ignorance. He said the apparatus was from the "Gaslight Improvement Company, Limited," and was called the "Economic Illuminator." It was never so called; and there is not now, nor ever was there, such a company! He stated that the carburetting vessel was "an air-tight metallic box." On the contrary, it is freely open to the air at all times through the burner, and, when in use, to the gas-main through the inlet-pipe! He stated that I told my audience "that a heat greater than that of boiling water was required to vaporise naphthalin," and that naphthalin is "the substance I propose to employ." I never told the audience that naphthalin was the substance I proposed to employ; for I employed and *showed* them *fluid coal oils* (naphthalin is a crystalline solid), and at their request plunged lighted paper, tapers, &c.; into the fluid to show them its non-inflammability at ordinary temperatures. I never told them that naphthalin required a heat greater than boiling water to vaporise it; but I *did* tell them that, like camphor, naphthalin volatilised at ordinary temperatures, if left exposed to the air in crystals! He stated that "naphthalin is highly inflammable, and when enclosed in an air-tight box over a gas-burner it may explode and ignite, and cause much damage." The whole of this is erroneous. I say that if naphthalin (or water) were enclosed in an air-tight box over a gas-flame it *must* (not *may*) explode, and that when it did it would not burn, and would not cause any damage. I state that naphthalin is *not* highly inflammable, and that when it is boiling, and is blown through a pipe by a current of air and ignited, it will not continue to burn! I have made this experiment, and so have other people, scores of times. The whole of the modern chemical doctrine of substitutions is based upon the classic researches of Laurent upon naphthalin and its derivatives. This distinguished chemist says, that when "thrown into a red-hot crucible, it volatilises undecomposed (*i.e.* does not ignite), and condenses in the air in snowy spangles."

All this is pardonable ignorance compared with the following:

"The economy of the carburetting process, if naphthalin is to be the carburettor, is doubly doubtful. Though a great nuisance to the gas manufacturer, naphthalin is difficult to obtain in large quantities, and it must be a very large gaswork indeed in which a ton of it could be collected in one year. The scarcity of the supply would put a stop to its use." It appears the only naphthalin the editor knew anything about was that which he hears of sometimes as stopping up the London gaspipes, though, even in regard of that, he could not manage to be accurate. The chairman of a London gas company kindly offered to give me two or three cartloads, deposited from his gas, if I chose to send for it, and had not naphthalin been a substance readily obtainable, I should have availed myself of his offer. As it was, I had a very large quantity at command. The difference between the writer and the chairman is very great; but then the latter is a gentleman, adheres to truth, and knew what he was talking about.

What are the facts about naphthalin? "Coal tar contains a quarter of its weight of naphthalin." * "Naphthalin is obtained in large but varying quantities as an almost useless bye-product in the distillation of tar on the large scale, especially of that from coal." "It may be useful to state that this interesting substance may be procured in *enormous quantities* at many of the tar-works, where it is deposited mixed with paranaphthalin, by the oils distilled from the tar, in granular crystalline masses, called 'salts' by the workmen. It is there thrown away as useless, or, at best, burned for lampblack." † "It is a vexatious circumstance that no important practical use has yet been found for naphthalin. It is true that it is used for the preparation of lampblack, but the quantity employed for that purpose is but small. *The quantity annually produced by the various gasworks is enormous.*" ‡

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* Gmelin, vol. xiv. p. 3.

† Mansfield. Journal of Chemical Society, vol. i. p. 268.

‡ C. G. Williams, in Ure's Dictionary—Art. "Naphtha," 1860.

Nothing is further from my thoughts than charging this writer with a knowledge of chemistry or of chemical literature; but he might fairly have been thought to have at least a slight acquaintance with "The Chemistry of Gas-Lighting.* That publication says, in speaking of the distillation of the fluid pitch obtained in distilling gas tar, "first water and naphtha, with a kind of eupione or volatile oil, make their appearance . . . *much contaminated with naphthalin.*" "The first of these products is almost *worthless, from the amount of naphthalin* it contains." "Permit the product to stand some hours in a cool place, so as to favour the *deposition of naphthalin,* which must then be separated from the oil." "The oil should next be steamed for some hours for the purpose of *dissipating the naphthalin.*" "Where the oil contains *much naphthalin,* the latter is the best method of the two." "*The great impediment* of purification *seems to be naphthalin in a form which is not disposed to crystallisation.*" "For the other ingredients of coal tar—that is to say, *naphthalin,* &c., no use has yet been devised; consequently, in a marketable sense, they are worthless." These extracts are taken within the space of eight pages. Do they intimate the scarcity or the abundance of naphthalin?

Already many tons of this scarce article have been sold for and consumed in my apparatus, and no doubt thousands of tons more will be burnt before it is very much older.

It is surprising that so much general ignorance should prevail upon the subject of gas tar. That substance has been produced as a matter of commerce for more than half a century, and for many years has been an important article of trade. Chemists have worked much with some of its constituents, and our text-books are rich in information about it; yet, so lately as 1860, we find the following passage under the head of *coal naphtha*, in a book written exclusively on "the Chemistry of Artificial Light:" "As the distillation of the tar proceeds, a heavy oil, which falls to the bottom of water, and is hence termed dead oil, comes over. This is used, under the name of creosote, for the preservation of timber. After this a yellowish semi-crystalline fat, called naphthalin, makes its appearance; and finally a more solid material, named paranaphthalin, distils over. That which remains in the retort is pitch."

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* London: Office of the Journal of Gas-Lighting, &c., 11, Bolt-court, Fleet-street, E.C., 1860.

It is an entire mistake to say that oil which will sink in water precedes naphthalin in the distillation of "dead oil." Naphthalin comes over during the entire distillation, and hardly a sample of commercial benzole can be obtained which does not contain naphthalin, although the boiling point of the latter substance is 410° F., and of the former but 176° F. The oils which boil below naphthalin are given below, and it will be seen that they are all lighter than water, and therefore cannot sink in that fluid:

	Boiling point.		Specific gravity.
Benzole	176° F.		·85
Toluole	230°	(Mansfield)	·87
Cumole	284°		·86
Amyle	311°		·77
Cymole	338°		·85
Caproyle	395°		·75
Naphthalin	410°		1·04

Then, again, it is a mistake to represent paranaphthalin as the only substance which comes over after naphthalin. I have had *tons* of oils distilled, which have a boiling point above that of mercury, and which consequently cannot be measured by ordinary thermometers, and also a large quantity of oils which boil between 420° F. and 600° F. From these higher oils I have obtained paraffin, chrysene, and pyrene, but have never yet obtained paranaphthalin with the properties assigned to it in books. I have purchased what the tar distillers call by this name, and have worked upon as much as 20 lbs. weight at a time, but I have never yet obtained what is described under the name of paranaphthalin. It may exist, of course, and if so I have been unfortunate in my material.

The following illustrations show some of the forms of apparatus made for adapting my principle to general use.

No. 1 shows a section through the hydrocarbon vessel, and thus affords an exemplification of the principle. The dotted lines show the hydrocarbons which are heated by the burning gas. N shows the pipe by which gas is admitted to the vessel, and H that by which it and the vapour are carried to the burner; I shows how the pipe is carried over the burning gas, that the gas and vapour may be kept hot and condensation prevented; O is an aperture through which the hydrocarbon vessel is filled.

Nos. 2, 3, 4, 5, and 3 *a* and 3 *b*, show various pendent apparatus.

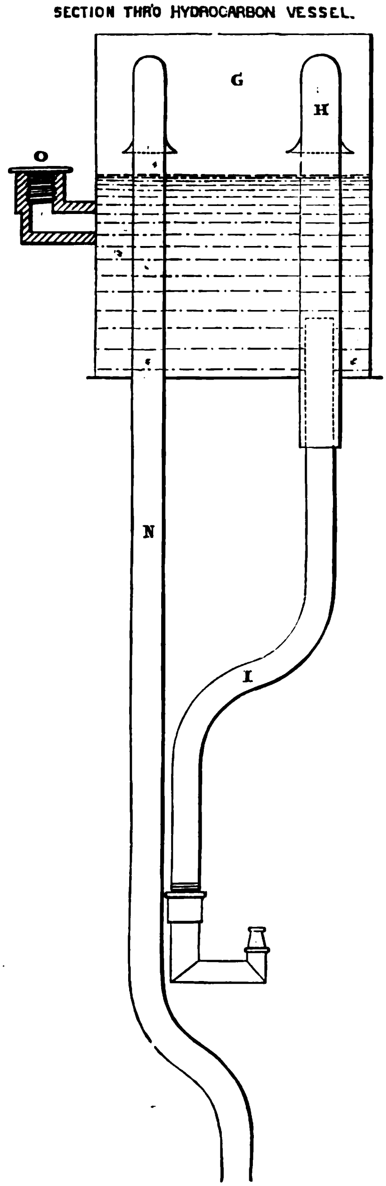
Nos. 6, 6 *a*, apparatus to fix upon standard pillars.

No. 7, apparatus used in street lighting.

No. 8, apparatus placed above floor with ventilating cone.

Nos. 9, 10, 11. Forms of chandeliers containing carburetting apparatus.

No. 12, apparatus applied to a table-stand.



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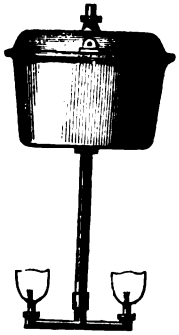

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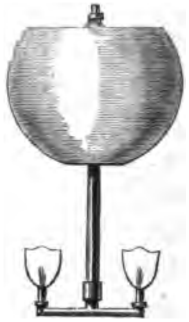

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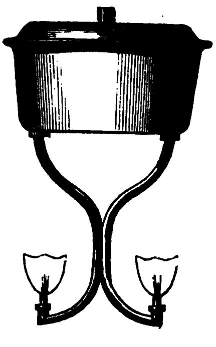

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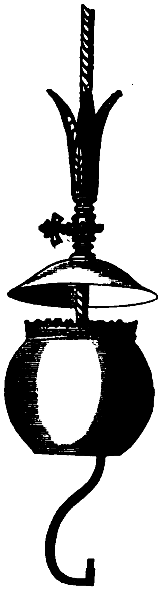
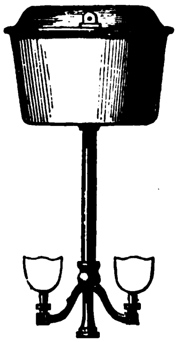


3 *a*



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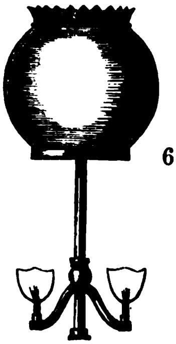

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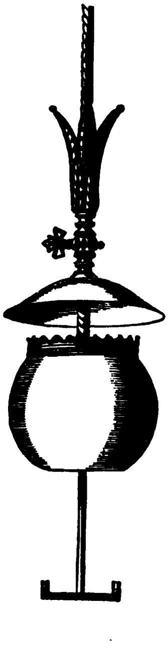
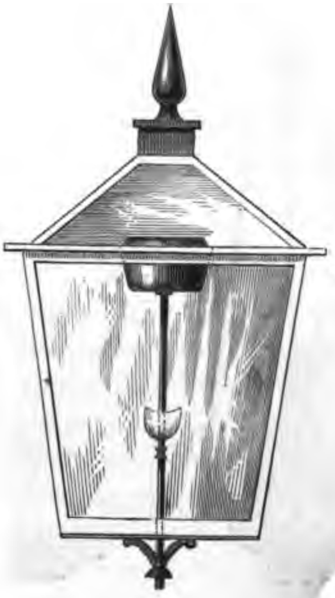


3 *b*



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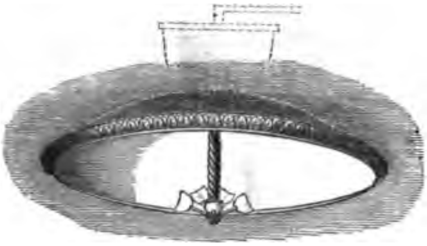

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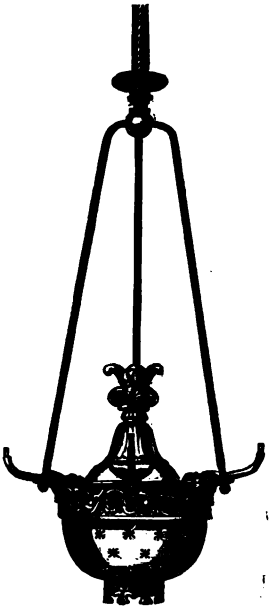

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12

RESULTS OF EXPERIMENTS ON THE CARBURATION OF COAL GAS, BY H. LETHEBY, M.B., ETC.

Last year Dr. Letheby published a summary of results, obtained in the carburetting experiments made in the city of London, which is useful as pointing out the value of carburetting gas, and indicating in some measure the causes of failure of the many people who have attempted to carburet upon wrong principles. I have abridged the paper slightly, but in no case have I altered its sense. "The principle involved in the process is, beyond all question, a correct principle, in so far as it relates in its application to the very inferior gas of London; but the success of the process is dependent on certain conditions which have not always been fulfilled. It has been ascertained, in fact, that the results of the process are affected by a great number of circumstances, many of which have been either disregarded, or have been placed beyond our control. These circumstances are the quality of the naphtha, the time of the experiment, the temperature of the apparatus, the form of the carburetter, the quality of the gas, and the rate at which it traverses the instrument. All these circumstances have received attention, and the following are the results of it:

1. As regards the quality of the naphtha.

This has a very marked influence on the illuminating power of the naphthalised gas. In point of fact, I have ascertained by experiments, not only that the different naphthas of commerce furnish different proportions of vapour to the gas, according to their different degrees of volatility, but also that those vapours are endowed with very different degrees of photogenic power. A single grain, for instance, of some of the hydrocarbons added to each cubic foot of common gas, will raise its illuminating power to the extent of only about 1·69 per cent., whereas a grain of other hydrocarbons will raise it nearly 9 per cent. This is illustrated by the following Table, where only a few of very many experiments are recorded:

Specific gravity, water being 1000.	Boiling point (Centigrade) deg.	Amount per cent. by distillation.		Average quantity taken up by each foot of gas. Grains.	Per-centage increase of illuminating power.	
		Up to 180° Cent. Grains.	From 130° to 150° Cent. Grains.		Total.	For each grain of naphtha per cubic foot.
698	63	86	14	20·1	33·9	1·69
676	40	98	2	34·4	62·1	1·80
869	102	76	22	12·1	40·8	3·37
827	115	56	40	6·5	21·7	3·43
808	117	27	53	6·1	21·0	3·60
852	128	4	51	3·8	14·2	3·72
869	107	75	19	9·2	34·9	3·79
869	103	83	14	11·8	46·8	3·96
816	119	15	45	3·4	14·4	4·23
856	114	23	49	4·4	18·9	4·29
814	105	60	34	70·0	33·5	4·78
865	124	9	34	3·3	15·8	4·79
845	90	92	8	12·0	65·3	5·44
874	119	45	37	4·8	26·7	5·56
879	93	92	8	9·5	53·2	5·60
870	129	5	44	2·8	15·7	5·61
862	121	10	45	3·3	20·4	6·16
848	97	77	15	10·2	68·4	6·70
875	110	75	20	6·9	60·8	8·81

It would seem from this that a naphtha with a low specific gravity and a low boiling point is not well suited for the carburation of gas; for although it yields a large quantity of vapour to the gas, the photogenic value of it is but small. This is the kind of naphtha obtained from the petroleums and shales of commerce, and it has not unfrequently been found in the naphtha supplied to the public lamps. On the other hand, the naphthas obtained from coal tar are much richer in carbon, and are, therefore, better suited for the carburation of gas. But the more volatile of these naphthas are so largely used in the manufacture of red and purple dyes, that it is difficult to obtain them at a reasonable price for the street carburettors. I have therefore been compelled to specify a quality of naphtha which is not well fitted for the manufacture of dyes; but this also is of rather low volatility, and is, therefore, not so good for the carburation of gas as it ought to be. The terms of the specification are, that the naphtha shall be colourless; of a specific gravity of about 870; of a boiling point not higher than 110° C. (230° F.), and yielding, on distillation, at least 70 per cent. of volatile naphtha between the boiling point, and 130° C. (266° F.), and 20 per cent. between 120° and 150° C. (266° and 300° F.). This naphtha, the last in the Table, furnishes about 7 grains of hydrocarbon vapour to each cubic foot of gas at ordinary temperatures, and it raises the illuminating power of the gas about 60 per cent.; in other words, it increases the light of a 3 ft. bat's-wing burner from about 7 candles to 11, and thus makes 3 ft. of gas of the same value as 4·8. If it were possible to obtain at a reasonable price a coal naphtha of a little higher volatility at ordinary temperatures, as, for example, a naphtha yielding about 10 grains of hydrocarbon to each cubic foot of gas, the illuminating power of the gas will be increased about 68 per cent. A gallon of this naphtha will weigh nearly 60,000 grains, and it will be sufficient to naphthalise 6000 cubic feet of gas, making them equal to about 10,000 ft. of unnaphthalised gas. Such a naphtha, even at six shillings per gallon, would be equal to just three times its money-worth of gas; for a gallon of the naphtha costing six shillings would give the light of 4000 cubic feet of gas, costing eighteen shillings. The only difficulty at present in the realisation of this object is in the uncertain composition of the naphthas of commerce; but this difficulty would soon be overcome, if a steady demand for such a naphtha existed. Even as it is, it may be said that the common coal naphthas of commerce increase the illuminating power of the London gas to the extent of about 4·5 per cent. for every grain of naphtha taken up by a cubic foot of gas. These naphthas are obtained at a maximum price of four shillings per gallon, and a gallon will double the illuminating power of rather more than 2600 cubic feet of gas; in other words, we have four shillings-worth of naphtha doing the work of about twelve shillings-worth of gas. These are the unquestionable results not merely of laboratory investigations, but also of carefully conducted experiments at the public lamps, Moorgate-street.

2. As regards the time or duration of the experiment.

As all the naphthas of commerce are mixtures of various hydrocarbons of different degrees of volatility, it happens that the most volatile constituents of the naphthas are given up very freely to the gas at the commencement of the experiment, and the less volatile, with more difficulty, at the end. In consequence of this, there is always a large increase of illuminating power when the carburettor is first charged with naphtha, and a marked diminution of it at last. This irregularity has not, hitherto, been compensated for by a regulated supply of gas; and therefore it has happened, that when a naphtha yielding, at first, as much as twenty-three grains of hydrocarbon per cubic foot of gas has been used, the illuminating power has been doubled; but, after a time, from the diminished volatility of the naphthas, the power has gradually fallen to less than 25 per cent. Numerous experiments have been made to determine the influence of this circumstance on the value of the process, and the following Table is given in illustration of it:

*Table showing the differences in the volatility of the Naphtha, and the illuminating power of the Gas, at different periods of the Experiments.**

Quantity of gas passed (cubic feet).	Quantity of naphtha taken up per foot of gas (grains).	Illuminating power in standard sperm candles.		
		Not carburetted.	Carburetted.	Increase per cent.
80	23·2	6·78	13·68	101·7
152	21·6	7·42	14·30	92·8
136	17·6	7·45	13·24	75·1
232	15·6	7·25	12·10	67·0
198	11·6	6·80	10·19	50·0
249	11·5	7·04	10·44	48·3
285	11·5	7·17	10·81	50·1
330	7·4	7·62	10·04	31·8
451	7·3	7·18	9·60	33·7
2113	12·1	7·21	11·60	60·1

3. The influence of temperature on the carburation of the gas.

In warm weather, the volatility of the naphtha is increased, and therefore a larger amount of hydrocarbon vapour is given to the gas:

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* This experiment was made with a street carburettor of the form designed by me, and with 26,000 grains, or nearly 4 pints of common naphtha, having a density of 869°, and a boiling point of 103° C., and yielding 82·7 per cent. of distilled product from its boiling point to 130° C., and 14·3 per cent. from 130° to 150° C. The experiment was continued for 30 days and nights, and the gas was continually burning from a three-foot bat's-wing burner, the temperature of the room being 22° C. (72° F.).

Table showing the volatility of the Naphtha and per-centage amounts of increase of illuminating power, according to temperature, at different seasons of the year.

Season.	Temperature. Fahrenheit.	Quantity of naphtha per cubic foot of gas (grains).	Per-centage increase of illuminating power.
Spring	41°	5·25	23·6
Summer	72°	12·09	54·4
Autumn	62°	10·77	48·5
Winter	37°	4·94	22·2

These results show that it is of great importance to keep the carburettors at as uniform a temperature as possible.

4. The quality of the gas affects the carburation.

When gas is already charged with a large proportion of the richer hydrocarbons, it manifests no disposition to take up the vapour of naphtha; on the contrary, if it be a cannel gas of very high illuminating power, the naphtha will deprive it, to a certain extent, of its hydrocarbons, and so render it weaker; the process, therefore, is only applicable to such a poor gas as that which is supplied to the City; and even in the case of this gas, it has been noticed that when a dose of naphtha has been already given to it, it does not show the same tendency to absorb the vapour as it did before."

[This paragraph, No. 4, is entirely erroneous, if it be meant to convey the idea, that rich gases cannot be enriched by carburetting them. In London, the cannel gases of the Western and Chartered Gas Companies are nightly increased in illuminating power from 150 to 200 per cent. A friend who owns a gasworks, where the standard of illuminating power conformed to in practice is 27 candles, informs me that his gas is raised from 27 to 39·8 candles by carburetting it with one of my apparatus, and considerably higher results have been communicated from Edinburgh, where the average illuminating power for the last three months has been 29·58 candles.* By passing gas over benzole, and then through one of my carburettors, I have obtained higher results than by using the gas alone without the benzole vapour. This must be the fact, if hydrocarbons unite chemically with each other, and with gases, as there is good reason for asserting.]

"In conclusion, therefore, it may be said, that although the success of the carburetting process is evidently dependent on many conditions, yet, as all these conditions are under control, there is no reason that they should not be at all times fulfilled, and the success of the process secured. One thing, it will be observed, is beyond all question, and that is, that every grain of common coal naphtha given to a cubic foot of gas increases its illuminating power about 4·5 per cent. (a good naphtha will increase it about 8 per cent.); and this grain of naphtha costs only about one-third of its equivalent in London gas."

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* Carefully-made experiments communicated to me show that one foot of Edinburgh gas, carburetted by my apparatus, is equal in illuminating power to three feet of the same gas uncarburetted.

CHAPTER XII.

ACETYLENE.

When coal gas is passed through an ammoniacal solution of cuprous chloride, an insoluble brick-red precipitate falls, and can be separated and examined. This precipitate is called acetylide of copper, or cuprous acetylene, and consists of a gas called acetylene, which has left the coal gas of which it formed part, and has become united to the copper. By treating this red precipitate with hydrochloric acid, the acetylene is set free from the copper, and may be collected and examined. It is a gas of an offensive odour, which burns with a very luminous flame, and has the formula $C_4 H_2$. It certainly adds to the illuminating power of the gas in which it exists, but the quantity hitherto found in gas is so small, that we cannot regard it as of much practical importance; and as the quantity is variable as well as small, we are not able to do more than state generally that it is a valuable constituent of gas. Though we cannot assign it much practical value *at present*, it has the highest scientific interest, and the progress of scientific investigation may at length enable us to produce it at a cost and in quantities which will render it a matter of commercial importance. Meanwhile, the reader may be put into possession of the following important facts relative to acetylene:

ACETYLENE FORMED BY SYNTHESIS. BY M. BERTHELOT.

"Carbon and hydrogen are capable of uniting directly. It is sufficient to bring the carbon to a state of incandescence by the electric arc, and cause a stream of hydrogen to pass between the poles. The hydrogen then immediately enters into combination with the carbon, giving rise to a gaseous hydrocarbon called acetylene:

$$C_4 + H_2 = C_4 H_2.$$

"The experiment is performed in a glass globe, having two tubulures opposite one another. Through one of these tubulures is inserted the positive pole of a voltaic battery, together with the tube which conveys the hydrogen; through the other are introduced the negative pole of the battery and a tube by which the hydrogen passes off.

"The two poles of the battery are terminated by pieces of the charcoal from gas retorts; they can be pushed forwards or backwards within the globe, and are in connexion with a Bunsen's battery of from 40 to 90 pairs. The essential condition of success is the production of the voltaic arc; the brighter it is, the greater will be the quantity of acetylene obtained. Before bringing the poles together, a current of hydrogen is passed through the globe for about a quarter of an hour, in order to drive out the air. The poles are then brought together, the arc shoots forth, and the production of acetylene immediately commences, continuing as long as the arc is maintained. About half the carbon lost by the poles is converted into acetylene, the rest being dispersed in the form of fine dust, which collects on the inner surface of the globe.

"To afford ocular demonstration of the formation of acetylene, it is sufficient to pass the gas which issues from the globe into a bottle containing an ammoniacal solution of cuprous chloride. This reagent absorbs the acetylene, giving rise to cuprous acetylene, $C_4 Cu_2 H$—a compound distinguished by its insolubility and its brick-red colour.

"From this cuprous acetylene it is easy to obtain acetylene itself in the pure state by the action of warm hydrochloric acid. Acetylene is then evolved, and cuprous chloride produced:

$$C_4 Cu_2 H + Hcl = C_4 H_2 + Cu_2 Cl.$$

" Acetylene is, as you observe, a colourless gas which burns with a very smoky flame. It is a very important gas, being invariably produced when organic substances are heated to redness. It is a constituent of coal gas, to which it communicates its rather fœtid odour and illuminating power."

ACETYLENE TRANSFORMED INTO OLEFIANT GAS.

"To transform acetylene into olefiant gas, it is sufficient to fix upon the acetylene two equivalents of hydrogen:

$$C_4 H_2 + H_2 = C_4 H_4.$$

"This hydrogenation is easily effected by treating cuprous acetylene with hydrogen. The hydrogen must, however, be generated, not in an acid solution, which would attack the acetylene, but in an alkaline liquid, which has no action on that compound. I avail myself, for the purpose, of the action of ammonia upon zinc. By this means the acetylene is transformed into olefiant gas." — *Journal of the Chemical Society*, vol. xvii., pp. 37-39.

THE FORMATION OF ACETYLENE IN INCOMPLETE COMBUSTION. BY M. BERTHELOT.

"Acetylene is formed, as I have already proved, at the expense of most organic compounds when submitted to the prolonged influence of a red heat. I now purpose showing the formation of this same carbide under a condition not less general; namely, incomplete combustion.

"The following is a general experiment proving this fact, either with gas or with very volatile liquids:

"Take a gas, such as ethylene, $C_4 H_4$; hydrochloric ether, $C_4 H_5 Cl$; propylene, $C_6 H_6$; methylic ether, $C_2 H_2 (C_2 H_4 O_2)$; marsh gas, $C_2 H_4$, &c., or, better still, a very volatile liquid, such as ordinary ether, $C_4 H_4 (C_4 H_6 O_2)$; amylene, $C_{10} H_{10}$; hydride of amylene, $C_{10} H_{12}$; and even benzole, $C_{12} H_6$; acetone, $C_6 H_6 O_2$; methylformic ether, $C_2 H_2 (C_2 H_2 O_4)$, &c.

"Fill a test tube of 300 cubic centimetres' capacity with gas, or pour into it a few drops of volatile liquid; then add a few cubic centimetres of ammoniacal cuprous chloride; set fire to the combustible vapour, and incline the tube almost horizontally, rolling it about, so as to spread the cuprous reagent over all the interior; cuprous acetylide will speedily be produced. It is produced by the contact of the flame from beneath, and is seen in the form of a characteristic red precipitate.

"This experiment answers especially well with ordinary ether and hydride of amylene.

"The amount of acetylene produced under these circumstances in the form of acetylide is evidently larger than that produced under the influence of heat alone, acting on the same compounds. The quantity of acetylene actually produced is, moreover, much greater than that which takes the form of acetylide, because the greater part of the acetylene burns almost as soon as formed, and without coming in contact with the reagent. I think it possible to deduce from this experiment, conveniently modified, a method of preparing acetylene more advantageous than those hitherto in use. Coal gas is no exception to this general rule, as may be easily ascertained by observing the traces of acetylene it contains in its normal state.

"I have, on the contrary, obtained no result, either with a mixture of oxide of carbon and hydrogen or with hydrogen charged with pure carbon dust, or directed in a jet on a stick of retort coke.

"Not only is acetylene formed during combustion, in a half-covered vessel of hydrocarbonated compounds, but it is produced and discharged into the atmosphere whenever an organic compound burns in contact with the air with formation of lampblack.

"This may be seen by placing the mouth of a tube over the flame, far enough off not to interfere with the combustion, and by drawing away the gases produced by the aid of a slow flow of water (one or two litres per minute). A dry empty flask, of about a litre, is thus filled by displacement with the gases of the combustion. After several minutes, put into the flask a few drops of ammoniacal cuprous chloride, and the characteristic precipitate will be produced, though much less abundantly than in a half-covered vessel. I have especially obtained this result by burning the following bodies in a small capsule:—ordinary ether; benzole, $C_{12}\,H_6$; oil of turpentine, $C_{20}\,H_{16}$; petroleum; vegetable oil; stearic acid; naphthalin, $C_{20}\,H_8$.

"The results given by benzole and naphthalin merit attention from the fact that I have hitherto failed to transform these carbides into acetylene by heat alone.

"This is another proof of the extreme stability of acetylene, particularly when mixed with a certain proportion of foreign gases.

"The same observations are applicable to the combustion of coal gas. The gases discharged into the air, whether by the flame of a burner known as the bat's-wing, or by the smoky flame of one of Bunsen's burners, contain a notable proportion of acetylene.

"These observations will explain the odour observable in rooms where gas is burnt. However, acetylene in the air exercises, by itself, no specially pernicious physiological action; for I have ascertained, by an experiment, in which Mr. Arm Moreau was kind enough to join me, that its poisonous action is not more marked than that of ordinary carbides of hydrogen. But its presence is an evidence of incomplete combustion; such a combustion should produce a considerable proportion of carbonic oxide, to the injurious properties of which M. Leblanc has testified.

"From the point of view of the theory of combustion, the general formation is not without interest. In the first place, it is contrary to the positive axiom that the whole amount of the hydrogen of hydrocarbons is completely burned during incomplete combustion, leaving free carbon.

"Now in the imperfect combustion of naphthalin, a body much less hydrogenised than the acetylene, $C_4 H_2$, which it engenders, it must be admitted that a portion at least of the original carbide loses its carbon before its hydrogen, $C_{20} H_8 = 4 C_4 H_2 + C_4$.

"In fact, the combustion of hydrocarbonated bodies is not effected at once, but by a series of decompositions. The first of these decompositions causes the formation of special products, depending on the particular nature of the combustible bodies. The first product of the incomplete combustion of alcohol, for instance, is aldehyde. Then come the general products, formed during all combustions, and preceding the water and carbonic acid. Carbon and carbonic oxide are the only two general products of this nature hitherto known; to these my experiments have added acetylene."—*Comptes Rendus*, LXI. iiii.; *Chemical News*, vol. xiii., pp. 77, 78.

PREPARATION OF FORMYL (OR ACETYLENE ?).

"Kletinsky fuses together under naphtha 25 grammes of mercury and 10 grammes of potassium. The dried amalgam rubbed to powder, and heated in a flask with 10 grammes of pure chloroform, sets free a litre of formyl, $C_2 H$. According to Fittig, the so-called formyl is always acetylene, $C_4 H_2$."—*Chemical News*, vol. xiii., p. 155.

FORMATION OF ACETYLENE FROM MARSH GAS AND CARBONIC OXIDE AT HIGH TEMPERATURES.

"Another new fact in connexion with gas requires to be noticed in passing, and that is, the discovery of a new illuminating constituent of coal gas, recently made by M. Berthelot. This illuminating agent is a gaseous substance, called Acetylene, which is produced under very peculiar circumstances. Unlike all other hydrocarbons with which we were formerly acquainted, an intense heat is favourable to the production of this body, and Dr. Odling has recently demonstrated that two of the constituents of coal gas, light carburetted hydrogen and carbonic oxide, which are useless for lighting purposes, may, by means of the strong ignition of the two gases, be made to form this acetylene, and thus become luminiferous agents. This quite revolutionises our ideas of the manufacture of gas. We have generally considered it important to preserve a moderate degree of heat in gas-making, in order to prevent the destruction of the other luminiferous agents; but the discovery of the formation of acetylene will render it necessary to investigate how far the production of this substance can be carried on upon a large scale, and rendered valuable for increasing the illuminating power of gas. The subject is yet in embryo; but it has an important bearing upon the future progress of gas-lighting. It burns with the most intensely luminous flame. Acetylene and olefiant gas contain in equal volumes the same amount of carbon, but the former contains only half as much hydrogen as the latter; consequently, the illuminating power of acetylene is, probably, at least double that of olefiant gas."—*Dr. Frankland, Lecture at The Royal Institution,* Feb. 13, 1863.

Dr. Odling has kindly informed me that up to the present time he has been unable to carry this matter further. It is to be hoped that so able a chemist will have leisure and inclination to pursue his researches into this obscure and difficult subject.

FORMATION OF ACETYLIDE OF COPPER IN COPPER TUBES FOR CONVEYING LIGHTING GAS. BY M. CROVA.

"It is well known that copper tubes, long used to convey lighting gas, have sometimes, during the cleansing of the interior, occasioned very dangerous and sudden explosions, occasionally proving fatal to the workmen. In one of the scientific reviews, an accident of this kind is recorded as happening at Philadelphia.

"Acetylide of copper having the property of detonating by an elevation of the temperature or by a blow, and the presence of acetylene in lighting gas having been recently proved by M. Berthelot, I have tried to ascertain whether acetylene, in presence of air, could combine with copper, and so form acetylide of copper.

"By passing a mixture of air and moist acetylene through a glass tube containing bright copper turnings, this metal tarnishes rapidly, assumes iridescent colours, and finally becomes black; but as this alteration takes place only on the surface of the metal, it is very limited.

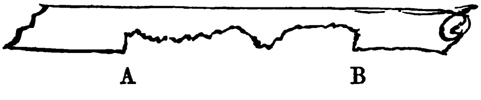
"By using copper reduced by hydrogen, I extended the surface of the metal. A small quantity of metal was placed in two flasks containing a mixture of equal volumes of air and acetylene, and also in one of the flasks a drop of ammonia. The two flasks, well stoppered, their necks plunged in water, were left to themselves for two days. They were then carefully opened, and the water entering them absorbed nearly half the gaseous volume. The absorption seemed to be somewhat greater in the flask containing a little ammoniacal vapour. The copper turned black. After washing and drying I found that it contained a notable quantity of acetylide of copper; for, heated with hydrochloric acid, it disengaged acetylene, and, thrown on a heated metallic plate, a louder explosion resulted than with pure acetylide, and without deposit of carbon. This difference will be readily understood, for the acetylide formed during my experiments finding itself in presence of excess of oxide of copper, the carbon and hydrogen of the acetylide were entirely destroyed.

"It is evident that copper, in presence of air and acetylene, is spontaneously transformed into acetylide of copper containing excess of oxide. As lighting gas contains acĕtylene, a little air, and perhaps even some ammoniacal vapours, it is obvious that acetylide of copper may be formed in tubes which have served to conduct lighting gas."—*From Comptes Rendus; Chemical News*, vol. vii., p. 3.

CHAPTER XIII.

EXPLOSIONS OF GAS. GNAWING OF GASPIPES BY RATS.

Many serious explosions of gas have occurred in dwelling-houses and shops which have never been accounted for. Plausible suppositions have been proposed to account for them, but I never recollect seeing them charged upon *rats;* and yet I have little doubt that rats have been at the bottom of some, if not of many, explosions which have appeared unaccountable. A short time ago a mansion near Wakefield was being fitted with gaspipes, and composition, *i.e.* drawn lead, pipes were laid as usual. Within a fortnight of the pipes being laid, and before the gasfitters had left the house, parts of the pipes were gnawed through by rats.



I saw one piece thus gnawed which had been cut out. The hole from A to B was nearly five inches in length, and in parts more than half the pipe in diameter was eaten away. The gasfitter informed me that there were several other holes in other parts of the pipes, though none so large as the one he showed me. He moreover added that it was no uncommon thing for lead gaspipes to be gnawed through in this neighbourhood, and specially instanced a railway station of which he does the fitting. If this kind of thing be common, it accounts for many an unexplained accident. Gas may be left turned off at the taps in all safety, and the fittings may be in every respect good, so that no smell was perceptible when the gas was extinguished for the night, and yet next morning the premises may be filled with an explosive mixture of gas and air. Rats would soon penetrate a soft metal like lead, and a single night of their work upon lead gaspipes might make an aperture large enough to produce frightful consequences, if a light were incautiously carried into the premises wherein the gas had been escaping.

Rats do not confine their performances in pipe-gnawing to gaspipes, but appear to be particularly addicted to pipes which convey beer. The keeper of a refreshment-room at a railway station found his sale of beer much increased, and yet the droppings from the taps, which were conveyed by a leaden pipe into a barrel in the cellar beneath the refreshment-rooms, were less in quantity than they used to be with a smaller sale. This continued for some time, and he determined to ascertain its cause. After some trouble, he found that the rats had gnawed a hole in the pipe before it reached the barrel, and thus intercepted for themselves what ought to have gone to increase his profit. Upon watching where he was not visible to the rats, he saw them come and drink the beer which passed through the hole they had made, and one of them drank so much that he was hardly able to walk away. Of course their performances ended with this discovery.* I do not introduce these as anecdotes about rats, but to afford light upon an obscure subject, and as giving a direction as to what should be sought in the case of an explosion before an opinion on its cause is risked. Blame may be cast upon innocent persons, when, in fact, it belongs exclusively to the mischief-loving rats. It may also suggest the wisdom of avoiding the use of lead tubes in buildings which are known to be infested by rats.

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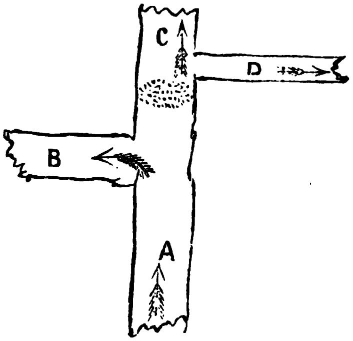
* I possess a piece of a lead pipe 2½ inches in diameter and ¼ of an inch thick, which has a hole in it which measures 2 inches by 1½ inch gnawed by rats. This came from the West Riding Lunatic Asylum.

CHAPTER XIV.

PRESSURE AS RELATED TO PUBLIC LIGHTING.

DISPUTES about *pressure* frequently arise between gas companies and towns' authorities, when contracts are made for a supply of as much gas as will pass through a certain burner at a fixed pressure. What is meant by such a contract? In cases of dispute how is the pressure to be ascertained? What *ought to be meant* by a contract of this character, is the *pressure exerted on the burner by the gas while it is being consumed.* Anything else but this is a delusion. I do not say that either of the parties to these contracts knows that they are delusive when they enter into them; but they are delusive nevertheless, whether they know it or not, unless the meaning be that the pressure is to be ascertained near the burner, while the gas is burning at the fixed rate. What happened in the town of Wakefield will exemplify this. The land rises a good deal from the south and west towards the north and north-east parts of the town, and at one time at the very highest part the complaints were continual about a deficient supply of gas, although other districts, at much lower levels, were well supplied. During the whole time of the complaints the pressure of gas at my own house was hardly ever less than from 18 to 22 tenths of an inch of water, and the supply of course abundant. Notwithstanding this, in the district whence the complaints came I found it difficult to read at a table under four lights burning, with all the taps turned on fully, and the gaselier drawn down as low as it would come. At home I could read with a single light so placed. The defective pressure was complained of, and the reply to the complaint was that it could not be well founded, as other parts of the town, fifty feet lower and further from the works, had plenty of pressure, and in the complaining district the pressure about one o'clock A.M., when all shops and domestic lights were extinguished, was found to be 15 tenths. I pointed out that the initial pressure at the works must have been enough, or other parts of the town could not be supplied as they were, and that the mains could not be stopped up, as some thought, or there would not have been the pressure which was found at night. The only solution was, that the mains were too small to allow each person to get a proper supply of gas during the time when all were burning, and this made both domestic and public lights more like candles than gas-lights. Upon examination it was found that the whole of an extensive district had but a *two-inch main* to supply it, and consequently that a proper supply to consumers was an impossibility. In this case, if the pressure had been taken near a burner when all lights were burning, it would have been found almost *nil*, though the initial pressure at the works could not have been less than 25 tenths, and though taken at nights, after house and shop consumption ceased, it was found 15 tenths throughout the complaining district. The fact was, that the apertures (burners), during the consuming hours, were able to pass out more gas than the mains could carry to supply them, and therefore pressure was impossible, but when most of those apertures (burners) were closed, and but few remained open, the mains were capable of passing more gas than the apertures could discharge, and pressure was the result. The mains then became fully charged; before they were not so. It is idle to talk of initial pressure at the works, and of the advantage of being at high levels, because these things have nothing to do with the matter unless the mains have sufficient capacity to pass enough gas to keep up a full supply to all burners during the time of consumption, and still to allow of residual pressure, which would be capable of supplying still more burners if they were put on.



Another case nearly as instructive as the above came under my observation here, and perhaps the same thing may have happened elsewhere. A new branch main, B, was put into the main A, which, as well as its continuations, C and D, was large enough to supply many times the quantity of gas which was actually drawn from it. For a long time C and D had supplied their districts abundantly; but after the new main, B, was put into the place of an old one which branched off to the same district, C and D ceased to supply more than a quarter of the gas which was actually wanted, and, when all lights were burning, the pressure of gas in the district was about one-tenth of an inch of water. I suggested that the main C was blocked up by the workmen when they laid the new main; but the idea was scouted as ridiculous, by those who ought to have known better, but did not. At length complaints became too loud to be neglected: the main was opened close to the junction, and a large mass of oakum, &c., was found, as shown by the dots between B and D, *which the men forgot to take out when they laid the new main!* I adduce this case to show that there may be causes of imperfect supply of gas which are never suspected to exist by those whose duty it is to remove them, and to suggest the propriety of submitting a complaint to a full, fair, exhaustive examination in all cases, instead of assuming an air of injured innocence, or of unapproachable superiority, and denying what may exist, and, if it do, cannot be affected by the most pertinacious and unscrupulous denial.

How can we determine what is really the pressure in a given instance? *i.e.* how can we ascertain whether the quantity of gas contracted for is received, or whether there is excess which robs the gas company on the one hand, or defect which robs the consumer on the other? Nothing is easier. Attach a pressure gauge to the lamps *while they are burning*, and determine the pressure carefully. Tabulate the variations from lamp to lamp, and then determine the quantity of gas which the burners in question will pass *while burning* at the pressures found at the lamps. The quantity consumed at each lamp can then be determined accurately, and by noting the pressure at one or more central stations, and keeping the burners clean, it would be easy to ascertain that the proper quantity of gas was given and received. This is not a consumer's question, nor is it a company's question. There is a mutuality of interest which ought to lead both parties to ascertain the truth, and the character for honesty of all concerned is involved in giving and receiving neither more nor less than what is paid for. I have said that the pressure should be taken when the lights are burning, not only on account of the influence which the sizes of mains, &c., may then exercise, but also because heat alters the apertures of burners, and the quantity which passes through a cold burner when the gas is not lighted is no measure of the quantity which will pass through that burner at the same pressure when the gas is burning, and the burner is fully heated. The difference is very considerable with some burners, and I have known it to be as great as ·44 feet an hour, upon a consumption of 5 feet hourly. The only satisfactory way of equitably settling such a contract as I have mentioned is to determine the pressure requisite to pass the agreed quantity of gas through the selected burner when the gas is burning, and to make that pressure the basis of the contract. The manner in which it is to be determined whether the contract is performed I have already indicated. Of course it is understood that the gas supplied is of a certain standard quality; otherwise the quantity which will pass a given burner cannot be determined by pressure merely. Variations in quality will alter the quantity which will pass through in a given time at a given pressure.

This question of pressure interests every private consumer of gas as well as public authorities. To be adequate in quantity, gas should be so supplied that every consumer may have upon his pipes when all his lights are burning a pressure of at least six-tenths of an inch of water, and whenever there is less than this it should be rectified, whether it proceeds from defective supply or defective fittings. I know cases where great inconvenience is suffered from deficient pressure in the mains. I also know others where the deficiency arises from fittings, especially from using meters which are too small.

THE END.

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